Journal of
Experimental Psychology

VOL. 30, NO. 1 JANUARY, 1942

AN EXPERIMENTAL INVESTIGATION OF PAST EXPERIENCE AS A DETERMINANT OF VISUAL FORM PERCEPTION [1]

BY MARY HENLE

University of Delaware

Experimental investigations of the rôles of past experience and of the autonomous or structural factors in the determination of visual form perception have yielded data which support conflicting views of the relative importance of the two kinds of determinants. Some investigators have failed to demonstrate the influence of past experience, while others consider it to be an important determinant of form perception. The work of Gottschaldt and of Braly may be cited to illustrate this situation. Gottschaldt (5) found that the setting up of a great deal of experience in perceiving a geometrical part of a complex figure was ineffective for the perception of the whole. Braly (1), on the other hand, found a significant influence of the figures of his impression series on the subsequent perception of a test series of similar figures.

Since the influence of both structure and past experience has been demonstrated under various conditions, one must assume the effectiveness of both kinds of factors in determining visual form perception. Then, a possible explanation of the confusing results obtained relative to this problem might be that past experience may be shown to be an effective determinant of form perception if non-experiential influences opposing it are relatively weak, whereas its effect may not be shown if such forces are too strong. This interpretation is consistent with the findings of Djang (2), who obtained results opposite

[1] The writer wishes to express her gratitude to Professor James J. Gibson, who suggested the problem, and whose direction and help throughout the course of the research and in the preparation of the report made possible their completion. She wishes also to thank Professor K. Koffka for suggestions and criticisms during the progress of the research. The greater part of this research was done at Smith College as a thesis for the Master's degree.

to those of Gottschaldt in an experiment which was essentially like Gottschaldt's except that the complex figures employed in it offered a number of different possibilities of organization. Djang found that subjects who had previously been trained with simple figures saw the latter as separate units of the complex figures in which they were embedded twenty times as often as control subjects who had had no such previous experience with the simple figures. The difference between Gottschaldt's experiment and Djang's lies in the different strengths of the structural forces opposing the experiential ones in the two cases. This difference must, therefore, account for the failure to demonstrate an effect of experience in the one case, but not in the other.

It is apparent that a critical demonstration of the influence of past experience on perception can be made only if structural and other non-experiential factors have been controlled. A number of previous investigators have attempted to investigate the effect of experience in isolation. This has been done by presenting the same test series of figures (*i.e.*, figures of identical structure) to control subjects without previous training and to subjects who had previously been given the specific training whose influence on the subsequent perception of the test series was to be investigated. The experiments of Braly (**1**) and of Djang (**2**), both of whom employed this expedient, have already been mentioned. Rubin (**11**), in his work on 'figural after-effect,' controlled structural factors in the same manner, by presenting the same figures to two groups of subjects who had previously had different kinds of experience in perceiving figure-ground patterns in accordance with different instructions. He found a tendency for subjects assuming a passive attitude to perceive figures in the same way as they had perceived them before under the influence of instructions to see either the black part as figure or the white as figure. Leeper (**9**) performed an analogous experiment with ambiguous figures. He found that, in 18 cases out of 19, subjects perceived a briefly exposed ambiguous picture in the same organization as they had previously perceived it. Another relevant finding of Leeper's is his demonstration that subjects who had been given previous exposures of fragmentary visual figures, as well as various helps in completing them, were able to complete the same figures correctly, when they were briefly exposed on a later occasion, in a higher percentage of cases than control subjects who had not been given previous training with them.[2]

[2] Mention of certain experiments of Köhler and von Restorff (**8**) and of Ortner (**10**) is relevant to the present discussion, since they employ a method for the investigation of memory which is based upon the fact that the traces of earlier perceptions facilitate similar later perceptions under unfavorable optical conditions. The main finding which these investigators obtained by this method is that the amount of this influence depends upon the extent to which serial conditions are favorable.

In the investigations which have been cited, an expectation on the part of the subjects to perceive in the test series the material which had previously been shown in the training series would have had the same results as those attributed to past experience. The attempt was therefore made to set up conditions which would prevent the operation of such expectations. It is, however, unlikely that Braly's results are entirely independent of the influence of set; and Djang points out that in her experiment there was evidence of an attitude of searching for the simple figures in the complex ones, although this set could not alone have been responsible for the advantage of the experienced over the control subjects in perceiving the simple figures in the complex ones. The investigation to be reported in this paper differs from the previous ones in that it contains an experimental proof that the results attributed to experience are independent of any expectation of the subjects to perceive the specific kind of test material employed.

The present study differs from previous demonstrations of the influence of past experience on perception also in another important respect. The experience whose influence on form perception was tested was taken from everyday life experience, rather than set up experimentally in a training series. The difference in frequency of experience between the experienced and the control material could thus be enormously greater than any such difference experimentally produced.

Under the conditions of the experiments to be reported in the present paper, past experience was studied as a determinant of visual form perception with the attempt being made to hold all other factors constant. The materials chosen for this investigation were those visual forms constituted on the one hand by letters, and on the other hand by their mirror reversals.[3] The former have been enormously often, and the latter very seldom experienced. The experiential determinant of perception will be defined, for the purposes of these experiments, in terms of this difference between letters and reversed letters.

Structural or autonomous determinants of perception will be defined as those arising from the internal organization of parts within a figure. Structurally, the obverse and reverse forms of any letter are identical except for orientation. The organization of parts within a figure is the same for the two; phenomenally, also, the two structures may be seen alike except for their orientation. There is thus a strong *a priori* probability that structural properties are held constant for the two members of a letter-and-reversed-letter pair, and that

[3] Actually, a few digits and their mirror reversals were also used. For simplification of statement, the term 'letters' will be used to mean letters and digits.

orientational differences as such will not make them behave differently with respect to the criterion of the effect of experience which has been adopted. Experimental evidence will be presented below in support of this probability.

The experiment consisted of presentation and reproduction of these two types of figures under conditions of reduced stimulation. The drawings were then scored for 'correctness.' The standard of correctness selected was the letter-form, obverse or reverse, as it would be printed by a typical subject; reproductions in which a figure was drawn in this way will be called 'positive.' The effect of past experience on perception would be demonstrated in the present case by a superiority of letters over reversed letters in frequency of positive perceptions. Should such a superiority appear, it could, of course, be attributed to the influence of experience only if control experiments showed that it was not the result of any other difference between letters and their reversals, nor of the expectation of the subjects.

A method of reduced stimulation was employed so that positive perceptions and reproductions could not be attained in 100 percent of the cases. This method, by reduction of the sensory data, allowed other alternative perceptions of a figure than the letter as it is normally drawn. If there is a difference in the readiness with which obverse and reverse letter-forms are perceived, then under these conditions it could be shown, whereas if the figures were seen as letters and reversed letters in 100 percent of the cases, no such difference could be made apparent. The method of reduced stimulation also offered the advantage of allowing forces other than the sensory data themselves a greater part in the determination of the phenomenal form than they would have under conditions of normal stimulation.

The problem with which we are dealing may now be formulated in terms of the specific experimental situation employed: *When it is possible for the actual perception to develop in a number of different directions, will letters, which have been present very frequently in perception, be perceived more readily than reversed letters, which are equivalent to letters with respect to structural properties, but which have been very seldom experienced?*

Method

Two methods of reduced stimulation were employed. In Experiments I, III, and IV, figures were presented to peripheral vision. Later, Experiment I was repeated, as Experiment II, and an additional control experiment, V, was performed under conditions of short exposure.

Apparatus for Experiments Using Peripheral Vision.—The apparatus used for these experiments was a perimeter consisting of a semi-cylindrical gray cardboard screen, 56 cm in height by 143 cm in semi-circumference, supported by a curved wooden frame, and resting on a table. The subject sat facing the screen with his head in a head rest which was clamped to the table 59 cm away from the center of the perimeter. The figures were exposed to the right eye of the

subject at a point in the right hand periphery. A circular hole (8.5 cm in diameter), through which the figures were to be shown, was cut in the cardboard 31 cm to the right peripherally from the center of the screen. Behind this hole wooden slots, into which the stimulus cards were slipped by the experimenter, were glued to the screen. A vertical cardboard screen in the saggital plane, cut to fit the subject's face, was attached to the head rest to prevent vision of the left eye in the right half of the field.

Three preparatory fixation points were placed at intervals in the left half of the perimetric field; a fourth 'critical' fixation point—the point fixated during exposure of the stimulus cards—was placed in the center of the field directly in front of the subject's eyes. The three preparatory fixation points were used to insure a gradual entry of the stimulus figure into the subject's field of vision, and to prevent the subject from overshooting in finding his fixation point. The degree of eccentricity from the fovea of the exposed figures varied from subject to subject over a range of 26° to 35°. This variation was produced by moving the central critical fixation point, which was attached to a sliding strip, slightly to the right or left. In this way, differences in the range of efficient peripheral vision of the subjects were roughly allowed for. If the subject saw too little, the eccentricity of exposure was decreased; if too clearly, increased.

A circular black spot, about 1.5 cm in diameter, could be moved about by the experimenter until it disappeared into the subject's blind spot. In this way there was provided a check on his fixation; all cases in which the subject reported glimpsing the black spot were thrown out. As an additional check, the experimenter could watch any eye-movements of the subject. A test of her acuity in detecting eye-movements showed that even such slight ones as were barely enough to bring the black spot into view could be detected.

The experiment was performed under artificial illumination, uniform over the field of the perimetric screen, provided by a 15-watt lamp placed just above the perimeter and by a light in the middle of the experimental room.

The stimulus figures were drawn in india ink on cards 13.5×11 cm, of the same color as the screen. Gray was used in preference to white in order to provide a smaller brightness difference between the figure and its ground—and thus to allow the figures to be presented farther in toward the fovea than would have been possible with black figures on a white ground. The letter forms ranged from about 20 to 24 mm in height and from about 12 to 19 mm in their widest part. They subtended a visual angle ranging from 2.5° to 3°.

The stimulus figures were drawn with broken contours. By rendering the forms somewhat different from the letter-forms as they are ordinarily drawn, the gaps in the figures were intended to increase the range of possible directions which perceptions might take. As explained above, the experience effect as it has been defined in these experiments could not be demonstrated under conditions which permit a very high percentage of positive perceptions. However, data from figures with complete contours are not different from results obtained with figures containing gaps, so in this respect the inclusion of gaps in the figures was an incidental factor in the experiment.

Procedure.—The subject seated himself before the apparatus, placed his head in the head rest, and fixated the central or critical fixation point. The experimenter then adjusted the marker to the subject's blind spot. The following instructions were given: "I am going to show you a series of figures in the periphery of your visual field. Without moving your head, fixate the point to the extreme left. This is to be your fixation point when no figure is being shown. As I count 'one, two, three,' you are to shift successively from this point at the left to the three succeeding fixation points to the right, never looking beyond the central point. The black spot should become invisible as you fixate the central point and remain so as long as you hold the fixation. Should it come into view, please report it. If you keep your fixation steady, the black spot will not appear. During each exposure I want you to describe briefly the figure that is being presented. At the end of each exposure I shall ask you to fixate again the point to the extreme left, after which I shall ask you to draw what you have seen." In no case was the subject told that he would be shown letters and reversed letters. Each figure was exposed for 4 seconds and then removed as the subject fixated to the left. Preliminary experimentation showed that a longer exposure period did not permit a better seeing of the figures, but in some cases caused them to be partially forgotten. Besides, the shorter exposure time reduced the amount of fluctuation of the figures. The data consisted of the drawings made by the subjects and their descriptions of what had been seen, which were recorded by the experimenter.

Apparatus and Procedure for Experiments Using Short Exposures.—The stimulus figures were exposed in a box, the front of which consisted of a milk glass plate. The glass plate was

uniformly illuminated by a neon light at the back of the box.[4] The stimulus cards were inserted in front of the glass plate with their blank side toward the subject, who could thus see the figures only when the box was illuminated by the neon light. The stimulus figures were seen surrounded by a gray cardboard frame.

The time of exposure of the stimulus figures was controlled by the self-calibrating time-control apparatus described by Helson and Powell (7).[5] By means of this apparatus, the circuit including the neon lamp could be broken after a desired interval. An exposure time of 60 σ was used in Experiment V and with half of the subjects of Experiment II. With the remaining subjects of this experiment, an exposure time of 20 σ was used. The shorter exposure time yielded, however, essentially the same results as the longer one. The results of all the subjects of Experiment II will therefore be considered together.

The subject seated herself at a table in front of the box in which the figures were to be exposed. She placed her head in a head rest which was 230 cm distant from the stimulus cards. The experiments were conducted in a darkened room. After each exposure of a stimulus card, the subject turned on a small lamp placed on her table and reproduced the figure she had seen. Then she fixated the center of the milk glass plate on which the next figure would appear. At the 'ready' signal for the next figure, she turned off the light, and the figure was presented. In case the subject missed a figure, it was presented a second time.

The stimulus figures were drawn in india ink on white cards 15 × 23 cm. The figures were 4 cm in their greatest dimension, so that at the distance at which they were seen, they subtended a visual angle of 1°, and thus fell well within the fovea. The figures were drawn with unbroken contours.

The following considerations apply to the procedure employed both in experiments using peripheral vision and in those using short exposures:

Each drawing was removed from the subject's sight immediately upon completion, to reduce to a minimum any influence of earlier on later figures. Whatever the extent to which such influence was present, it could not have affected the difference we are looking for between letters and their reversals, since the chances were equal for it to affect either.

Within each experimental group, the subjects were subdivided into two equal groups, the one being shown figures which were the exact mirror reversals of those shown to the other group. Each subject was thus shown some letters and some reversed letters, but never both members of any one pair. This method was adopted to prevent the influence of one member of an obverse-reverse pair on the other.

Preliminary experiments and Experiment III to be reported below show that when the subject is set to see letters or reversed letters, the results obtained are quite different from those obtained when the expectation is neutral. For this reason, the crucial figures (the letters and their reversals) were presented serially interspersed among nonsense figures in order to reduce the possibility of the subject's developing a letter-expectation. In Experiments I, II, III, and V to be reported below, the main series was also preceded by a preliminary series of forms, given partly as a practice series, partly to determine roughly the range of peripheral vision of the subject, and also to set up in the subject the desired expectation.

Subjects.—Experiments I, III, and IV were performed at Smith College. After some preliminary experimentation, 127 students and instructors were used as subjects in the main experiments. Experiments II and V were performed at Bryn Mawr College. In addition to preliminary subjects, 43 students acted as subjects in these experiments.

Scoring.—A reproduction of a letter-form was scored as positive if it was made as people print the letter or reversed letter in question, regardless of whether the letter was actually named or not. The latitude allowed a correct response was determined on the basis of standards set by an examination of drawings by 13 additional subjects who were asked to print the letters and reversed letters used in the experiment. Even though this procedure entailed a certain amount of arbitrariness, it seemed justified, because the same criteria were applied to both kinds of figures. Therefore, any difference between them cannot be due to the principle of scoring employed.

---

[4] The apparatus has been described in greater detail by Fehrer (3).

[5] The writer wishes to thank Professor Harry Helson for devising and setting up the apparatus for the experiments using short exposures.

Experimental Results

After some exploratory experimentation, Experiment I was performed with 30 subjects. They were shown a preliminary series of 7 nonsense forms as a practice series and, in this experiment, also to establish a 'neutral' set, *i.e.*, a set which was not an expectation to see either letters or reversed letters. These were followed by 6 crucial figures so embedded in a series also containing 10 nonsense forms that no crucial figure ever immediately succeeded another. The crucial figures shown to each subject were the obverse or reverse form of each of the following: K, N, 5, S, 7, and G. (See Chart I.) As explained above, no subject was shown both members of any pair.

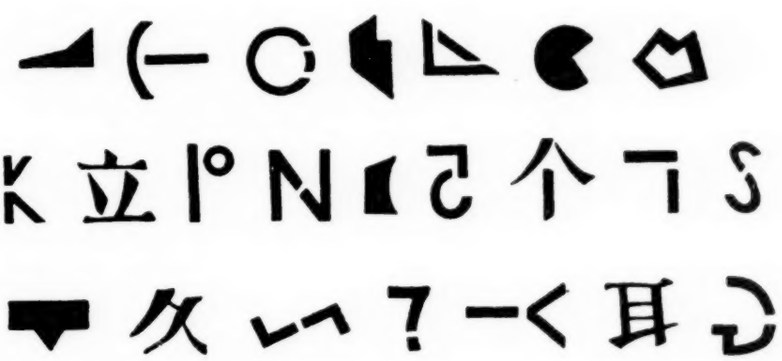


Chart I. Preliminary series—Experiment I. Test series—Experiments I and III.

The results of the experiment are presented in Table I. Forty-two percent of the letters presented, and only 16 percent of the reversed letters, were reproduced 'positively'—*i.e.*, as these letter-forms are usually drawn. The difference is 26 percent. This difference is significant, the critical ratio being 4.0.[6] A comparison of the frequencies of positive perceptions within the individual letter pairs shows no reversal of this direction of difference, although reversed 7 was perceived positively in the same proportion of cases as 7.

The results of this experiment are confirmed by those of a preliminary experiment with a different group of subjects, performed under conditions like those of the present experiment except for minor changes. The results were approximately the same as those here presented.

It is apparent from the results of Experiment I that under these conditions letters are more readily perceived than their less familiar reversals. Since this sort of differential result has been made the

[6] Sigma was calculated from the formula: $\sigma = \sqrt{\frac{pq}{N}}$, where p is the empirical probability of an event, $q = 1 - p$, and N is the number of cases.

TABLE I[7]

FREQUENCY OF POSITIVE PERCEPTIONS OF LETTER-FORMS IN EXPERIMENT I (NEUTRAL SET)

Letter	Score	Cases	%	Letter	Score	Cases	%
K	10	15	67		3	14	21
N	9	15	60		1	15	7
5	6	15	40		4	14	29
S	4	14	29		1	15	7
7	5	15	33		5	15	33
G	3	15	20		0	15	0
Total	37	89	42	Total	14	88	16
Sigma: .052				Sigma: .039			

Difference: 26%
C.R.: 4.0

criterion of an effect of experience in these experiments, we may tentatively conclude that the influence of past experience on the perception of visual form has been demonstrated.

EXPERIMENT II

Experiment II was designed to repeat Experiment I under conditions of short exposure. It is conceivable that this method brings out the figural factor in perception to a greater extent than the method of peripheral vision, and that the new method might, therefore, yield different results.

Experiment II was performed with 40 subjects, none of whom had served in the previous experiment. As before, only one member of each letter-and-reversed-letter pair was shown to each subject; each subject saw, however, some letters and some reversed letters. The obverse and reverse forms of K, S, G, 2, 5, and 7 were employed.[8] The letter-forms were included in a series which also contained 40 nonsense forms.[9] The series was so arranged that each letter-form was separated from the following one by no less than 5 nonsense

[7] There are sometimes 14 cases and sometimes 15 because occasionally a case had to be thrown out because the subject reported seeing the black spot or the experimenter detected eye movements of the subject.

[8] The letter-forms were different from those of Experiment I in that they were drawn with unbroken contours, while those of the previous experiment contained gaps. It has been mentioned above, however, that the two kinds of figures were found to yield the same results when both were tested under conditions of peripheral vision.

[9] The nonsense forms employed were selected from those used by Fehrer (3). They were chosen because it was desirable, for reasons not relevant to the present discussion, to know the difficulty of the forms. These nonsense figures are not reproduced here since the data from them will not be reported in the present paper.

figures. The test series was preceded by a practice series of 10 nonsense forms.

The results of Experiment II are presented in Table II. Letters were reproduced correctly in 79 percent of the cases, reversed letters in 58 percent. The difference of 21 percent is statistically significant, the critical ratio being 3.62. As in Experiment I, a consideration of the individual letter pairs shows no case in which a reversed

TABLE II

FREQUENCY OF POSITIVE PERCEPTIONS OF LETTER-FORMS IN EXPERIMENT II (NEUTRAL SET)

Letter	Score	Cases	%	Letter	Score	Cases	%
2	10.5	20	53	Ƨ	8.5	20	43
5	18.5	20	93	ƨ	16	20	80
7	17.5	20	88	٧	14.5	20	73
G	16	20	80	Ↄ	8.5	20	43
S	17	20	85	ƨ	11.5	20	58
K	15	20	75	ꓘ	10.5	20	53
Total	94.5	120	79	Total	69.5	120	58
Sigma: .037				Sigma: .045			

Difference: 21%
C.R.: 3.62

letter was reproduced positively more frequently than its obverse form. These results, then, fully confirm those of Experiment I in showing the advantage of letters over reversed letters in perception,[10] and thus in showing what has tentatively been called an effect of past experience on the perception of form. The final interpretation of these results must, however, depend upon the results of the control experiments which follow.

CONTROL OF ATTITUDINAL FACTORS

Gottschaldt (6) failed to demonstrate an automatic influence of experience *per se* on the perception of his figures. However, he did find that "the inner situation of the subject at the time of the test, which was determined by the actual course of events, might be decisive for the occurrence of an effect of experience, thus only apparently automatic" (p. 5). In view of these findings, we must show that attitudinal factors could not have produced the apparent

[10] The difference in the absolute percentages of positive reproductions between Experiments I and II is to be attributed to the difference in ease of perception under the conditions of the two experiments.

experience effect of Experiments I and II before the interpretation of these experiments becomes clear. Experiment III was designed to rule out any possible letter-expectation of the subjects as an explanation of the results of Experiments I and II. Sixty subjects, none of whom had participated in any previous experiment, were shown the test series used in Experiment I. Half of them—Group A—were first given a preliminary series of 7 letters, and the other half—Group B—were first shown a preliminary series of 7 reversed letters. (See Chart II.) These preliminary series were given to set up in the

CHART II. Preliminary series—Experiment III-A.[11]

subjects expectations of seeing like forms in the remainder of the experiment. Except for the preliminary series shown them, conditions were identical for subjects of Experiment I ('neutral' set) and for Groups A and B of Experiment III; the test series and the conditions of presentation of the figures were the same for all groups.

TABLE III

FREQUENCY OF POSITIVE PERCEPTIONS OF LETTER-FORMS IN EXPERIMENT III-A (LETTER SET)

Letter	Score	Cases	%	Letter	Score	Cases	%
K	7	15	47	ꓘ	8	14	57
N	7	15	47	ᴎ	5	13	38
5	4	15	27	ƨ	2	14	14
S	4	15	27	Ƨ	4	13	31
7	3	16	19	⁊	2	14	14
G	3	15	20	Ↄ	3	16	19
Total	28	91	31	Total	24	84	29
Sigma: .048				Sigma: .05			

Difference: 2%
C.R.: .29

The results of Experiment III appear in Tables III and IV. Group A (letter set) perceived letters positively in 31 percent of the cases, and reversed letters in 29 percent of the cases, giving a difference of only 2 percent. The critical ratio is .29; the difference is thus insignificant. Group B (reversed letter set) saw letters posi-

[11] The mirror reversals of these letters constituted the preliminary series of Experiment III-B.

tively 45 percent of the time and reversed letters 34 percent of the time, making a difference of 11 percent. This difference is not significant either, the critical ratio being 1.51. Thus the difference in readiness of perception of the two kinds of letter-forms practically disappears when subjects are set to see either letters or reversed letters.

When only the first two crucial members of each series are considered, and the sets are presumably operating in purer form, this tendency for the difference between letters and reversed letters to

TABLE IV

FREQUENCY OF POSITIVE PERCEPTIONS OF LETTER-FORMS IN EXPERIMENT III-B (REVERSED LETTER SET)

Letter	Score	Cases	%	Letter	Score	Cases	%
K	8	15	52		10	14	71
N	10	15	67		8	15	53
5	5	13	38		2.5	15	17
S	7	15	47		4	15	27
7	5	15	33		4	15	27
G	5	15	33		2	15	13
Total	40	88	45	Total	30.5	89	34
Sigma: .053				Sigma: .05			

Difference: 11%
C.R.: 1.51

disappear becomes even more marked. The number of positive cases becomes almost identical for letters and reversed letters under these conditions; for Group A (Table III) the difference becomes 1 percent (C.R. : .08), and for Group B (Table IV) it becomes 2 percent (C.R. : .15).[12] Furthermore, in Experiment I when only the first two crucial figures are considered, and the 'neutral' set is presumably uncomplicated by the development of any letter or reversed letter expectation, the difference between letters and reversed letters positively perceived rises from 26 percent (when the whole series is considered) to 49 percent (C.R. : 4.45). To a lesser degree the same thing happens when only the first half of each series is considered.

[12] This result is due in part to the exclusion of digits in this comparison. When only letter-forms are considered, the difference between obverse and reverse forms in Experiment III is somewhat reduced. This result indicates that the letter and reversed letter sets apparently do not carry over to digits. It is consistent with Ortner's finding (10) that an expectation to perceive words actually introduced errors in the reading of numbers. With such a set, subjects tended to read as words those numbers which bore some resemblance to words.

Discussion

The results of Experiment III show conclusively that an expectation of the subject to see letters (or reversed letters) could not have been responsible for the observed difference in frequency of positive perceptions of letters and their reversals, since such an expectation tends to destroy this difference rather than create it. The experience effect demonstrated in Experiments I and II is therefore independent of such sets.

The findings of Experiment III suggest, however, another possible interpretation of Experiments I and II. It may be that the 'neutral' set of the first two experiments is not truly neutral, but that a positive set to see nonsense figures is essential for the appearance of the experience effect as it is defined in these experiments. Subjects set to see nonsense forms may tend to see as nonsense forms the less familiar members of the letter pairs. If this is the case, the difference found in Experiments I and II between letters and their reversals would still be a function of experiential rather than of attitudinal determinants as such, because it would still be true that the more familiar obverse members of the letter pairs were seen as nonsense figures in a significantly smaller percentage of cases than the less familiar reverse members.

Experiment III presents another problem of interpretation: Why should the difference obtained in the previous experiments between letters and reversed letters disappear with the establishment of a different kind of set?

A comparison of the results of Experiment I (Table I) and Experiment III (Tables III and IV), experiments which differ only with respect to the sets established in them, shows that the disappearance of the difference between the two kinds of letter-forms in Experiment III is a consequence of the increase in the positive perceptions of the reversed letters under these conditions. Reversed letters were perceived positively significantly more often in Experiment III (taken as a whole) than in Experiment I; the critical ratio is 3.02.

These results present an exact analogy to certain results of Köhler and von Restorff (**8**). The latter investigators assume that pair formation between a process and a trace in the brain field is the necessary condition of recognition and reproduction of the trace. The process is assumed to enter into pair formation with a trace similar to it (*i.e.*, a trace laid down by a similar process), in the same way that similar items form pairs in the perceptual field. Köhler and von Restorff (**8**) found that spontaneous reproduction and recognition of the critical trace occurred frequently in experiments in

which serial conditions favored the necessary pair formation between the process and trace. On the other hand, recall occurred much less often under serial conditions unfavorable to the pair formation in question. The difference between the favorable and the unfavorable series disappeared, however, when subjects were set for recall, *i.e.*, when a vector existed in the direction of the critical trace. As in the present case, the disappearance of the difference between the two series was the result of an increase in the percentage of positive cases in the unfavorable series. Köhler and von Restorff conclude (**8**, p. 66) that a 'searching in the trace field' makes pair formations possible under otherwise unfavorable conditions.

Correspondingly, in the present case, the influence of past experience on perception may be conceived of in terms of such communication between the process of a stimulus figure and the trace of a letter-form. Thus, pair formation between the process and trace in question occurred, in the case of the reversed letters, significantly more often when a set to perceive reversed letters introduced a vector in the direction of the trace (Experiment III-B) than when no such vector existed (Experiment I). As in Köhler and von Restorff's experiments, the expectation made possible pair formations which did not occur spontaneously.

It is interesting to note that the letter set (Experiment III-A) acted in the same way as the reversed letter set in increasing the positive perceptions of reversed letters. Such a result would be understandable if one assumed that the trace with which the process communicates, in the case of reversed letters as in that of letters, is the trace of the obverse letter-form. The fact that reversed letters are known and named through their obverse forms lends plausibility to this assumption; phenomenally, reversed letters are letters 'turned over.' If, then, communication between the process and the letter trace is the basis of positive perceptions of reversed letters, then any vector in the direction of the letter trace, whether it is produced by a letter expectation or by a reversed letter expectation, should increase the percentage of positive perceptions of these forms.

In terms of such a view, the reason why letters are perceived positively more frequently than reversed letters under conditions of neutral set is apparent. The pair formation occurs more readily in the one case than in the other because the similarity between process and trace, which is a condition of such pair formation, is greater in the case of letters than in the case of reversed letters. Only when a vector is directed to the trace does pair formation occur frequently under the less favorable conditions of similarity.

If the letter and reversed letter sets increased the percentage of reversed letters positively perceived, the question arises: why did

they not similarly increase the frequency of positive perceptions of letters? The failure of the expectations introduced in Experiment III to increase the positive perceptions of letters must be attributed to the difficulty of viewing under the conditions of these experiments. The figures were drawn with incomplete contour and were presented under conditions of reduced stimulation for the express purpose of making possible other alternative perceptions of the figures than the positive ones. Under these conditions it is obvious that the upper limit of possible positive reproductions of the letter-forms must be very considerably below 100 percent of the cases. The significant point is, however, that this limit was attained by letters when the expectation of the subjects was neutral, and by reversed letters only when an expectation to perceive letter-forms produced a 'searching in the trace field' for such forms.

Control of Structural Factors

Before the results of Experiments I and II can be interpreted as unequivocally demonstrating an experience effect, it must be shown that differences between letters and reversed letters other than the difference in experience could not have been responsible for these results. Letters differ from their reversals in their orientation as well as in the amount of experience subjects have had with them. Gibson and Robinson (4) point out that with some types of figures a shift in the orientation of a form "may even render the form unrecognizable or cause it to appear to all intents and purposes a different object." Therefore, any possible structural differences between letters and their reversals must be shown not to produce any difference in the readiness with which they are perceived before the superiority of the obverse letter-forms in this respect can be attributed to their advantage in amount of past experience. The strong *a priori* probability of the structural equivalence of letters and reversed letters was pointed out above. This probability has been increased by the results of Experiment III, in which it has been shown that under certain conditions reversed letters are perceived positively as frequently as letters. The control experiments now to be reported lend further support to the assumption of the structural equivalence of letters and their reversals.

The method of Experiment IV was to expose figures and their reversals to subjects under conditions similar to those of Experiments I and III. The figures were 12 nonsense forms drawn to be as nearly as possible equally familiar or unfamiliar in obverse and reverse orientation. (See Chart III.) None of the figures, in other words, possessed one orientation which was dominant in meaning to the

other. They were drawn to be, in essential structure, however, as much like letters as possible to make them comparable to the forms used in Experiment I.[13] Yet, they did not 'look like' letters. Should the figures of Experiment IV be found to be perceived as readily in one orientation as in the other, and if these letter-like figures are truly comparable to letters, orientational differences would be ruled out as having been responsible for the results of the previous experiments.

Thirty-seven subjects who had served previously as subjects in exploratory experiments were used in Experiment IV. They were divided into two sub-groups; as before, the figures shown to one group

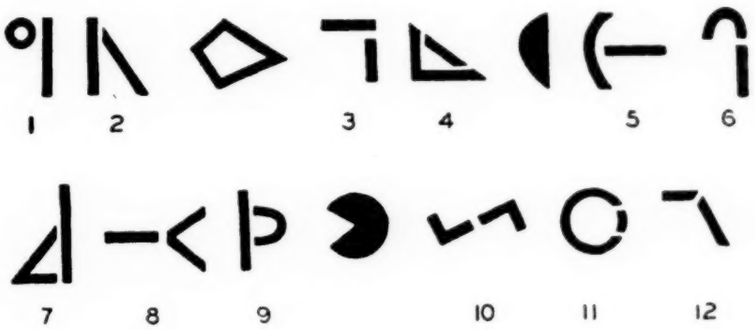


CHART III. Test series—Experiment IV.

of subjects were the exact mirror reversals of those shown to the other group. The procedure here was the same as in Experiments I and III except that the subjects were told at the beginning of the series that they would be shown nonsense figures. Additional data were obtained by including some of the obverse-reverse pairs of this experiment as nonsense forms in other experiments. These data are included in the results discussed here. Scoring in this experiment was analogous to that in the other experiments. A drawing was scored as positive if the figure was reproduced with closed contour, but was accurate in other respects. This method of scoring was employed to make the performance demanded of the subjects of Experiment IV as nearly as possible comparable to that demanded in the earlier experiments.

The figures used in this experiment were selected, as stated above, so as to have no orientation dominant in meaning to the reversed orientation. Therefore, it might be argued that it is an arbitrary

[13] Three additional unletter-like unbroken figures were included in the series to reduce somewhat the possibility of the subject's developing a 'gap set' or expectation to see figures with broken contour. Such a set, if it were very general, might reduce the reliability of the results by reducing the number of positive cases, since a reproduction was not scored as positive unless in it the gaps of the stimulus figure were closed. Data from these unletter-like figures are not included in the results, since they would not be relevant to the present problem.

matter which of the members of a pair is to be the obverse and which the reverse form. The results of the experiment will depend, of course, upon how the grouping is made. In so far as the grouping of the figures is arbitrary, this experiment is inconclusive. A grouping of the stimulus figures according to their resemblance to letters or reversed letters seems, however, to be the most relevant one for the present purposes, which are to determine whether or not letters and reversed letters are structurally equivalent.

TABLE V[14]

FREQUENCY OF POSITIVE PERCEPTIONS OF CONTROL FIGURES OF EXPERIMENT IV

Figures Resembling Letters				Figures Resembling Reversed Letters			
Figure	Score	Cases	%	Figure	Score	Cases	%
1(P)	38.5	63	61	1(ꟼ)	47	60	78
2(N)	12	18	67	2(И)	14	19	74
3(F)	35	62	56	3(ꟻ)	55	63	87
4(4)	26	34	76	4(ㄣ)	24.5	33	74
5(E)	27.5	33	83	5(Ǝ)	20.5	34	60
6(S)	9	19	47	6(Ƨ)	10	18	56
7(5)	12	19	63	7(ƨ)	14	18	78
8(K)	39.5	62	64	8(ꓘ)	42.5	64	66
9(P)	14.5	18	81	9(ꟼ)	14	19	74
10(N)	59	64	92	10(И)	58	63	92
11(G)	25.5	32	80	11(Ↄ)	27	33	82
12(7)	14	18	78	12(ᒣ)	10	19	53
Total	312.5	442	71	Total	336.5	443	76
Sigma: .021				Sigma: .020			

Difference: 5%
C.R.: 1.67

The stimulus figures were therefore grouped according to their structural resemblance to letters or reversed letters. Letter-like figures were seen positively in 71 percent of the cases, their reversals in 76 percent of the cases (Table V). The difference is one of only 5 percent; it is, furthermore, in favor of the figures resembling reversed letters. The difference is not a significant one, the critical ratio being 1.67.

[14] The figures are represented by the same numbers which they are given in Chart III. The letter in parentheses is in each case the one which the figure is taken to resemble. Although the same number is used to represent the obverse and the reverse form of any figure, it is to be understood that a number in the one column refers to a figure which is the mirror reversal of that to which the same number refers in the other column.

The number of cases varies from figure to figure because some of the figures were also included in other experimental series, while others were not, and because it was necessary to throw out cases in which the subject did not maintain a steady fixation.

Experiment IV has thus shown that geometrical forms which resemble letters are structurally equivalent to other geometrical forms which resemble reversed letters, with respect to the criterion employed in these experiments. These results increase the probability that those geometrical forms which constitute letters are structurally equivalent, with respect to this criterion, to those which consistute reversed letters.

It might still, possibly, be argued, however, that the obverse rather than the reverse orientation of a letter-form has become dominant in meaning because the former is the better structure. In other words, it may still be that letters were perceived positively in

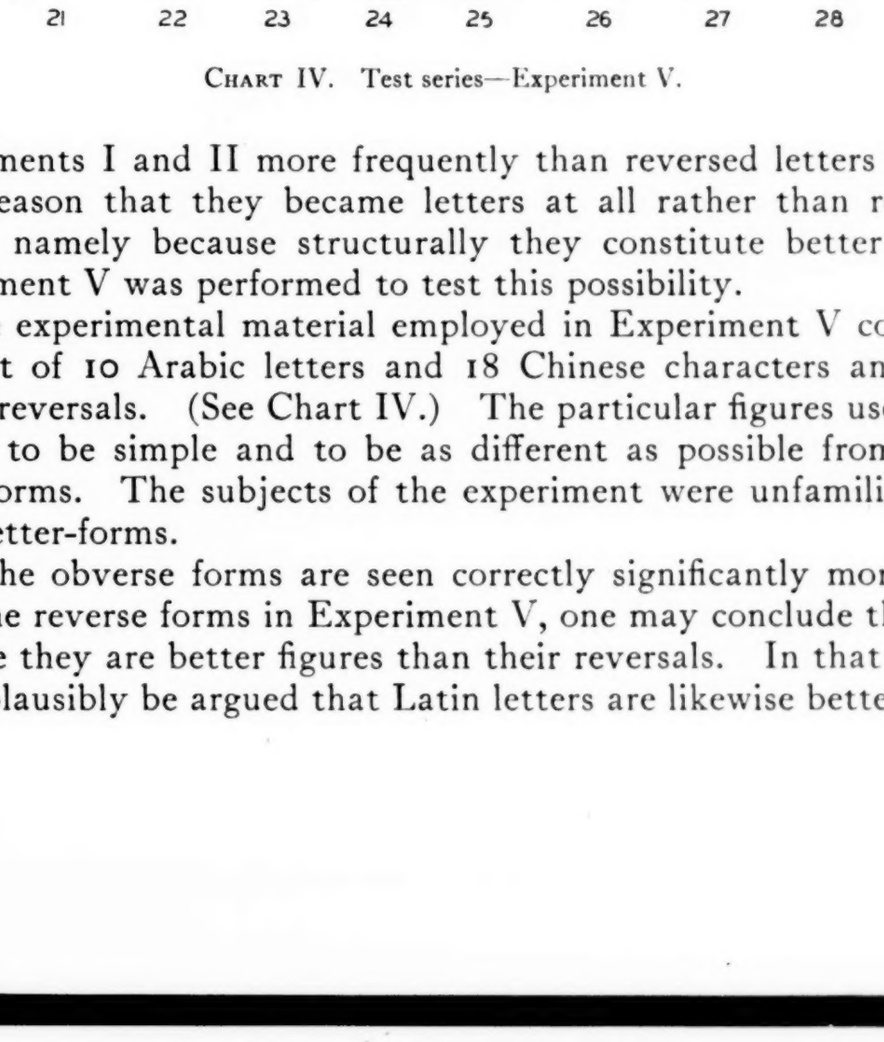


CHART IV. Test series—Experiment V.

Experiments I and II more frequently than reversed letters for the same reason that they became letters at all rather than reversed letters, namely because structurally they constitute better forms. Experiment V was performed to test this possibility.

The experimental material employed in Experiment V consisted of a set of 10 Arabic letters and 18 Chinese characters and their mirror reversals. (See Chart IV.) The particular figures used were chosen to be simple and to be as different as possible from Latin letter-forms. The subjects of the experiment were unfamiliar with these letter-forms.

If the obverse forms are seen correctly significantly more often than the reverse forms in Experiment V, one may conclude that it is because they are better figures than their reversals. In that case, it could plausibly be argued that Latin letters are likewise better struc-

tures in normal than in reversed orientation, and, indeed, that they have become dominant in meaning to their reversals for this very reason. If, on the other hand, no significant difference is found in the readiness with which the Arabic and Chinese letter-forms are perceived in the two orientations, one may conclude that they are structurally equivalent with respect to the criterion adopted. In that case, and by the same logic, the probability that Latin letters and their reversals are also equivalent in that respect will be enormously increased.

The procedure followed in Experiment V was identical with that of Experiment II. Sixteen subjects were used, all of whom had served previously in Experiment II or in a preliminary experiment. They were, as before, divided into two groups, the one group being shown the mirror reversals of the figures shown to the other. Half of the figures shown to each subject were obverse, and half reverse forms of the Arabic and Chinese letters employed; no subject, however, saw both members of any letter pair. The test series was preceded by a practice series of 10 nonsense forms.

Because the number of correct reproductions of the forms was extremely small, a rating scale was devised for scoring them. Ratings were assigned on a scale of 0 to 3 in the following manner:

0—reproduction completely unrecognizable
1—general form recognizable
2—reproduction containing minor errors
3—reproduction correct except for slight errors in the execution of the drawing.

Where it was impossible to decide between two ratings, a rating half way between was given. Subjective errors certainly enter in such a procedure as the present method of scoring, and it is likely that the standards varied from figure to figure; thus the same rating might indicate a better reproduction in the case of one figure than in that of another. But a figure and its reversal were always scored together, so that the same criteria would be used for the two forms. Thus it is unlikely that errors of scoring influenced the main result (*i.e.*, the presence or absence of a significant difference between figures and their reversals). The rating was done together with Dr. Marian Hubbell, and agreement with her was reached in rating each figure. To avoid the possibility of bias, the figures were rated without either of the raters knowing which form was the obverse and which the reverse member of a pair.

Table VI contains the results of Experiment V. The obverse Arabic and Chinese forms were given a total rating of 217 out of a

possible 672 (32 percent); the reverse forms were rated 210 out of a possible 672 (31 percent). The difference is only 1 percent, and is negligible, the critical ratio being .40.

The same results are obtained when the two kinds of letters are considered separately. Arabic letters were given a total rating of

TABLE VI [15]

SUMS OF THE RATINGS ON ARABIC AND CHINESE LETTER-FORMS OF EXPERIMENT V

Letter	Total Rating (24 possible)	Reversed Letter	Total Rating (24 possible)
1	5	1	3.5
2	12	2	11.5
3	9.5	3	10.5
4	11.5	4	11.5
5	12.5	5	10.5
6	5.5	6	5
7	3.5	7	2.5
8	6.5	8	3.5
9	6	9	9
10	7	10	9
11	7	11	7
12	11.5	12	8.5
13	5.5	13	5.5
14	5	14	9
15	9.5	15	9
16	8	16	7.5
17	7	17	2.5
18	2	18	1.5
19	8.5	19	2.5
20	7	20	6
21	6	21	4.5
22	7	22	11
23	8.5	23	11
24	10	24	9.5
25	9.5	25	8
26	10.5	26	11.5
27	8.5	27	10
28	7	28	9
Total	217	Total	210
Percent	32	Percent	31
Sigma	.018	Sigma	.018

Difference: 1%
C.R.: .40

79 out of a possible 240, or 33 percent; their reversals were rated 76.5, or 32 percent. The difference is only 1 percent, and the critical ratio only .23. Chinese characters were rated 138 out of a possible 432, or 32 percent; their reversals received a total rating of 133.5, or 31

[15] The figures are represented in the table by the same numbers which they are given in Chart IV. Although the same number is used to represent the obverse and reverse form of any figure, it is to be understood that a number in the one column refers to a figure which is the mirror reversal of that to which the same number in the other column refers.

percent. Again the difference is only 1 percent; the critical ratio is .31.

Experiment V has thus demonstrated that, for subjects unfamiliar with them, the obverse forms of Arabic and Chinese characters are structurally no better than their reverse forms. In view of this finding it becomes highly improbable that Latin letters are better structures than their reversals. It is predicted that such letters would likewise be found to be structurally equivalent in their two orientations for subjects unfamiliar with them.

Discussion

It has obviously been impossible to test directly the structural equivalence of Latin letters and their reversals, since the two types of figures are not equivalent, for the subjects of these experiments, with respect to the experiential determinant of perception. Indirect evidence of such equivalence may, however, be obtained by showing that letter-like nonsense figures are structurally equivalent to their mirror reversals, and that the obverse and reverse orientations of letters with which the subjects are unfamiliar are equivalent in this respect. Such evidence has been supplied by Experiments IV and V. These experiments thus indicate that the difference between letters and reversed letters, which has been found in Experiments I and II, is not to be attributed to structural differences between them. The only other difference between the two kinds of letter-forms lies in the amount of experience that subjects have had with them. Since Experiment III has shown conclusively that the advantage in frequency of positive reproductions of letters over their reversals is not a function of an expectation of the subjects to see these forms, it must be a consequence of this difference in past experience.

The question may still be raised whether the experience effect demonstrated here is an effect directly upon perception, or whether it is a matter rather of interpretation by the subject of ambiguous sensory data. It is conceivable that the letters are not superior to their reversals in actual perception, but that the unclearly perceived figures are interpreted as letters; the reversed letters may be no more unclearly perceived, but are simply not interpreted as the much less familiar reversed letters. That this explanation is unlikely is suggested by the fact that the difference between letters and reversed letters appeared under conditions of short exposure as well as in experiments using peripheral vision. Yet the ambiguity of the perceptual data, and hence the possibilities of interpretation, are much less in the former than in the latter case.

At any rate, we may conclude from the results of the present experiments that under conditions where it is possible for a perception to take any one of several possible directions, that direction will be favored, other things being equal, which perception has previously taken.

It should, finally, be pointed out that an influence of experience on perception has been demonstrated only in the sense of an influence of previous organizations on subsequent organizations appearing under less favorable circumstances. Past experience has not been shown to act by building perceptions out of nothing. Our conclusion applies only to the case of communication between a present process and an already existing trace of a previous perception.

Summary

Past experience as a determinant of visual form perception has been investigated with other determinants held constant. The following results were obtained:

1. Under conditions of reduced stimulation, letters were perceived positively significantly more frequently than their mirror reversals by subjects familiar with the letters.
2. An expectation on the part of subjects to see letters has been shown not to have produced the observed difference between letters and reversed letters. Indeed, such a set prevents this difference from appearing.
3. Further control experiments supplied evidence of the structural equivalence of letters and reversed letters. Nonsense figures constructed to resemble letters in structure were found to be structurally equivalent to their mirror reversals with respect to the criterion employed in these experiments. Likewise, Chinese and Arabic characters were no more readily perceived in obverse than in reverse orientation by subjects unfamiliar with them. Thus all the indications are that structural differences between letters and their reversals are not responsible for the difference in readiness of perception between them.

(Manuscript received May 29, 1941)

References

1. Braly, K. W., The influence of past experience in visual perception, *J. exp. Psychol.*, 1933, 16, 613–643.
2. Djang, S., The role of past experience in the visual apprehension of masked forms, *J. exp. Psychol.*, 1937, 20, 29–59.
3. Fehrer, E. V., An investigation of the learning of visually perceived forms, *Amer. J. Psychol.*, 1935, 47, 187–221.

4. GIBSON, J. J., AND ROBINSON, D., Orientation in visual perception; the recognition of familiar plane forms in differing orientations, *Psychol. Monogr.*, 1935, **46**, No. 210, 39–47.
5. GOTTSCHALDT, K., Über den Einfluss der Erfahrung auf die Wahrnehmung von Figuren. I, *Psychol. Forsch.*, 1926, **8**, 261–317.
6. GOTTSCHALDT, K., Über den Einfluss der Erfahrung auf die Wahrnehmung von Figuren. II, *Psychol. Forsch.*, 1929, **12**, 1–87.
7. HELSON, H., AND POWELL, N., A self-calibrating time-control for multiple circuits, *Amer. J. Psychol.*, 1937, **49**, 109–113.
8. KÖHLER, W., AND VON RESTORFF, H., Analyse von Vorgängen im Spurenfeld. II. Zur Theorie der Reproduktion, *Psychol. Forsch.*, 1935, **21**, 56–112.
9. LEEPER, R., A study of a neglected portion of the field of learning—the development of sensory organization, *J. genet. Psychol.*, 1935, **46**, 41–75.
10. ORTNER, A., Nachweis der Retentionsstörung beim Erkennen, *Psychol. Forsch.*, 1937, **22**, 59–88.
11. RUBIN, E., *Visuell wahrgenommene Figuren*, Copenhagen: Gyldendalske Boghandel, 1921.

MEMORY TRACE AND PERCEPTION IN THE BLIND[1]

BY L. GRANT TENNIES

Little Valley, N. Y.

Many investigations have been made concerning the changes of memory. Although the work of Ebbinghaus is thought of primarily as a study of learning and forgetting, it is equally important as a first study of memory. From the day of Ebbinghaus until the present time memory has been studied from the standpoint of memory span, the effect of associations on memory, the effect of sleep, drugs, etc., change of form or shape of memory-images, and many other phases.

Blankenship (**5**) in 1938 made a review of the work on memory and claimed that we have little real knowledge concerning memory. His criticism seems to be based on the following points. In none of the hundred and forty-odd references which he considered was there careful control of the experimental conditions that have a bearing on memory. He points out that material may be presented through any sense organ or combination of sense organs, and he cites studies to show that the more sense organs involved the better the memory. He emphasizes the fact that there is a difference in ability to remember between sexes, age groups, races, etc., and points to the widely diverse methods used to study memory, as well as to the vast differences in material.

Agreement or disagreement with the attitude of Blankenship (**5**) is of little importance at this point. He is referred to because he has proposed conditions apropos to this study, which will be considered later.

Late in the nineteenth century Baldwin and Shaw (**3**) set the stage for the study of memory for visual figures when they reported that the memory-images for squares decreased in size with the passage of time. At the present time it seems rather well understood by most psychologists that we do have a change of shape or form when certain types of visual or auditory stimuli are recalled. However, during the past few years there has been considerable discussion as to the results and conclusions of certain experiments (**1**, **10**, **13**, **17**). Perhaps the greatest amount of discussion has taken place between the

[1] This study was carried out at Louisiana State University under the direction of Dr. C. H. Bean. Acknowledgment is made to Drs. Paul C. Young and Allan Grinstead for kindly advice and helpful suggestions and to Mr. Y. L. Fontenot, President of the Louisiana State School for the Blind.

Gestaltists and those psychologists who disagree with the tenets of the Gestalt school of thought.

The points at issue in these discussions seem to center about the question of 'better' figures and 'progressive' change. By better figures is meant the tendency for the irregularities of a pattern to disappear or become regular; by progressive change is meant that the memory-trace of a figure should change progressively towards a better figure with the passage of time. Wulf (**17**) and Perkins (**13**) in their studies seem to find better figures and indications of progressive change. Allport (**1**) indicates shrinkage, *i.e.*, reproduced figures are smaller than the stimulus, and a tendency toward symmetry. Warner Brown (**6**) and Hanawalt (**10**) failed to verify the results of Wulf (**17**) and Perkins (**13**); and Woodworth (**16**: 82 ff.), considering the results of the studies, agrees with Hanawalt (**10**) and Brown (**6**). Fehrer (**9**) appears undecided, agreeing with one group on one point, disagreeing on another.

It might be pointed out at this time that the disagreement as to the results is not nearly so important as the reason why there are these disagreements. It would seem that the reason for the disagreements are quite apparent and hinge about two main points, the first being the method of study of the problem and the second the treatment of the results.

With respect to the first of these two points, namely the procedure of investigation, most of the experimenters have followed the same general procedure (**9**, **6**, **10**, **17**), that is, either the reconstruction method or the reproduction method. In either case the stimulus is presented to the subject, and at a later time he is asked to reproduce or reconstruct the stimulus from memory. The disadvantages of these methods are at once apparent. Is the reconstructed or reproduced figure what the subject remembers or is it an approximation (**8**: 448 ff.)? Might there not be details that would have been included provided the subject was better able to draw (**16**: 78)? As the memory becomes more and more vague, more details are lost. Questions by the experimenter may bring out these details; but if there are no questions and the subject must reproduce something, it seems logical to assume that instead of drawing a reproduction of the stimulus he might attempt to draw some figure that he has subconsciously associated with it (**16**: 84). It is a question as to whether the drawing is a reconstruction of a memory-pattern or a construction based on some fragment of memory. Carmichael, *et al.* (**7**), have shown the importance of suggestion on reproduced figures. From the standpoint of the above argument it would seem advisable to develop some method other than that of reproduction or reconstruction of visual memory-patterns.

However, it seems that as much discussion as taken place in regard to the interpretation of results as in regard to the method of obtaining them. This interpretation of results involves a subjective component in the study on the part of the experimenter. What might appear to be a case of progressive change to one experimenter, to another might be something else. This is apparent from a statement made by Woodworth (**16**: 89) in which he criticizes Perkins and Koffka: "Looked at casually this series of reproductions shows a gradual change toward symmetry. Close scrutiny shows some halting and some retrograde changes."

Another point under discussion is that of how the subject actually perceives the form at the time the stimulus is presented. Here again the matter of subjectivity raises its head. About the only information that we have concerning perception of forms comes from the introspective reports of subjects (**16**: 783 ff.). It seems to me that to be of value, introspective reports should be given by people trained for that purpose. I doubt if it would be possible to obtain *naïve* subjects trained to give introspective reports. As shown above, the subjective aspect would thus include, not only a matter of opinion on the part of the subject, but a matter of interpretation by the experimenter evaluating the reports.

With two notable exceptions the studies of memory, both from the standpoint of what is originally perceived as well as from the standpoint of memory-pattern, have been of subjective nature. The two exceptions are here noted: Bean (**4**) pointed out that perception of visual stimuli could be studied objectively by using blind subjects; Irwin and Seidenfeld (**12**) have shown that by using the same stimuli continuously in a comparison method of study of visual forms, the experimenters' interpretations could, to a large extent, be eliminated.

Statement of the Problem

It was the purpose of this study to determine objectively whether, with the passage of time, figures do become 'better' and whether the change of the memory-image is 'progressive.' It was further intended to make a study of memory which would eliminate the most flagrant of the criticisms put forward in the preceding pages.

Subjects

The subjects of this study consisted of two groups. The first group was comprised of twenty totally blind students, eighteen from the Louisiana State School for the Blind and two from the Louisiana State University. Blindness in all cases occurred before the fourth year. Eleven of the subjects had been blind since birth. Three were blind at the age of three years, three at the age of two years, and three at one year of age. Age range was from eight to twenty-four years, mean age sixteen and three-tenths years. The educational grades ranged from

pre-school[2] to the college sophomore level, mean grade between the seventh and eighth. The group contained nine females. With the exception of the pre-school child all subjects had received the same course in tactile training. The pre-school child had nearly completed this course of training.

Because of the poor standardization of intelligence tests for the blind (11), only subjective estimation of the intelligence could be made. This was supplied by the teachers of the School for the Blind. With the exception of one borderline and two subnormal subjects, intelligence ratings were judged to be normal or better.

The subjects of the second group were chosen after experimentation on the first group had been completed. Certain questions were raised by the experimentation on the first group, and the second group was picked to try to answer these questions. This group also consisted of twenty-four (sighted) subjects, twelve from the practice school of the Louisiana State University and twelve from the psychology classes of the University. The age range of the group was from seven to thirty-seven years, mean age level sixteen and nine-tenths years. The intelligence rating of the practice school group was rated normal or better by the teachers, and the remaining twelve subjects were considered above normal intelligence as they were of college sophomore level or above.

Material

Six patterns were modeled after the figures of Irwin and Seidenfeld (12). These consisted of the following: 1, open circle; 2, obtuse angle; 3, arc-line figure; 4, acute angle; 5, dot figure; 6, open triangle (see Fig. I). One set of figures was mounted in relief on veneer plyboards thirty centimeters square. With the exception of the dot figure, figures were made of wire three millimeters in diameter. The four dots comprising the dot figure were smooth-surfaced furniture 'gliders' fifteen millimeters in diameter. These figures were also duplicated on cards of the same dimensions as the boards. Measurement of the figures follow:

No. 1. Open circle: diameter, 146 mm; gap at top 80 mm.
No. 2. Obtuse angle: limbs each 106 mm; angle of 122 degrees, opening upward.
No. 3. Arc-line figure: horizontal line 109 mm; semi-circle attached to right extremity of line; diameter 100 mm.
No. 4. Acute angle: open upper right; lower limb horizontal; angle 34 degrees; upper limb 184 mm; lower limb 150 mm.
No. 5. Dot figure: four dots disposed as approximate square with eccentric upper right hand corner. Base 150 mm between centers of dots; left side 150 mm between centers of dots; base and left side at right angles; right side 180 mm; top side 180 mm.
No. 6. Open triangle: base 146 mm; sides 126 mm; width of gap 24 mm; figure bilaterally symmetrical with gap at top.

A hooded table was used with the sighted subjects which was constructed in such a manner that the experimenter could at all times see the action of the fingers and hands of the subjects but which prevented the subjects from seeing either his hands or the stimulus boards.

Procedure with Blind Subjects

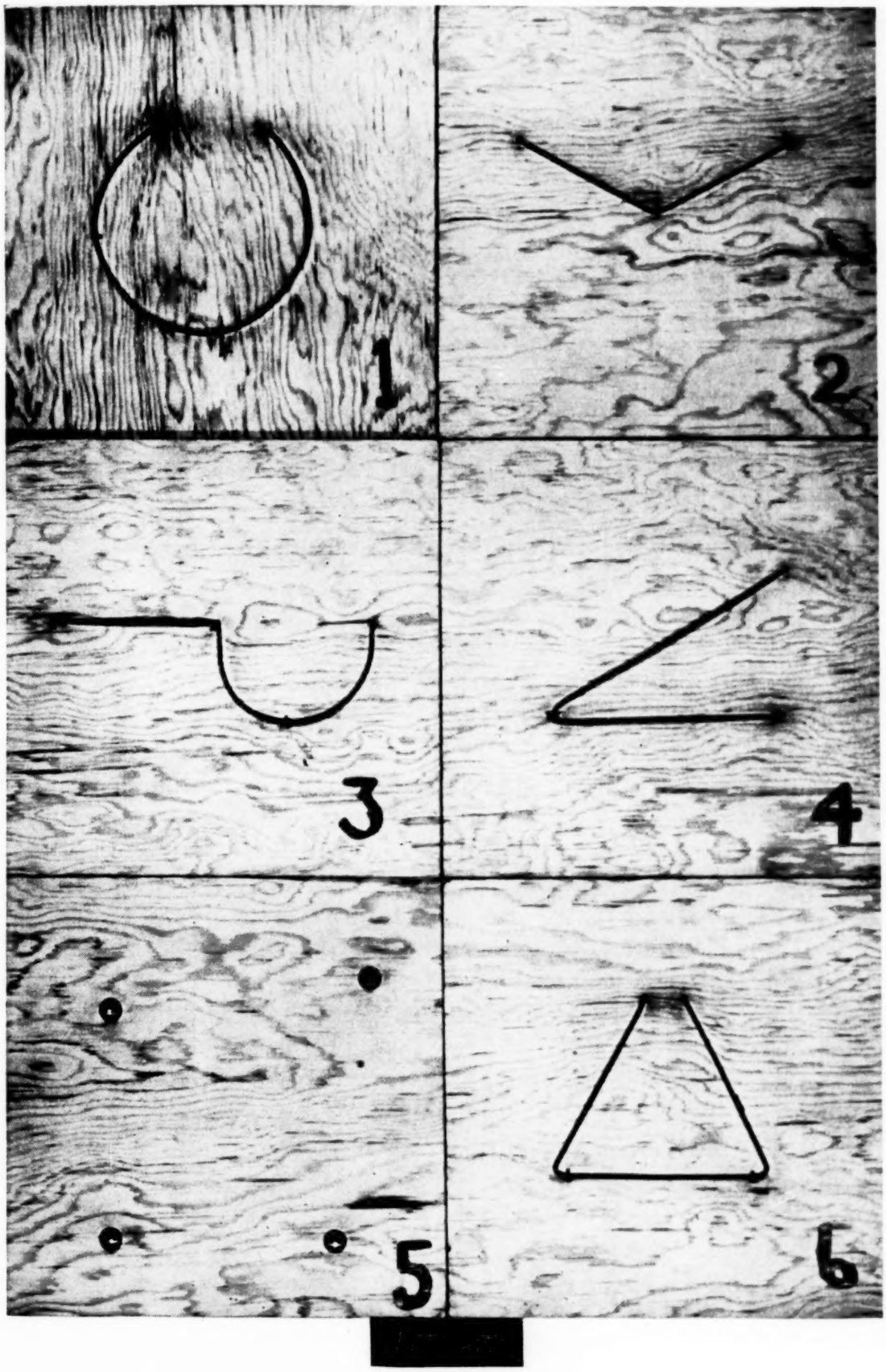
The successive comparison method was used in this study, modified to comply with the conditions of the experiment.

Each subject was seated at a desk in the usual classroom, and the stimulus boards were placed on the desk one at a time in the order tabulated above (p. 11). After the stimulus board was presented in the proper position, the subjects were given the following instructions:

> "I am going to show you several different figures. I want you to explore them carefully so that when I show you some similar figures, in the future, you will be able to compare them with these. Do not talk to your neighbors about these figures."

---

[2] The pre-school classification depends on the amount of training received. It is not an age classification.


1
2
3
4
5
6

The subject was then permitted to turn the board to any position desired until he was familiar with the pattern. In order to obtain the maximum perceptual effect an attempt was made to eliminate the memory factor by telling the subjects the boards would be similar. Because of the limited number of subjects with vast differences in chronological age no time limit was set for the exploration. This also avoided the problems of rhythm and rate of presentation.

The boards were re-presented four times in the same order as above and with the same instructions, at periods of five minutes, fifteen minutes, thirty minutes, and seven days.

In each case the instructions were as follows:

> "This is an experiment dealing with small differences. I am going to have you explore some figures more or less similar to those that you have already seen. As I show them to you I will ask you some questions about each figure. If you are undecided, please guess as well as you can. Remember to compare these figures with the first series of figures that I showed you."

With each presentation of the stimulus boards the instruction in the form of a warning to compare always with the first series of boards was added. The questions for each figure are as follows:

No. 1. "Is the gap wider or narrower?
Is the area greater or smaller?"
No. 2. "Is the angle greater or less?"
No. 3. "Is the line longer or shorter?
Is the arc more or less curved?"
No. 4. "Is the base line longer or shorter?
Is the angle greater or less?"
No. 5. "Is the area greater or less?
Is the figure more nearly square or less nearly square?"
No. 6. "Is the gap wider or narrower?
Is the area greater or less?"

Results for Blind Subjects and Discussion

The results of the experimentation with the blind subjects are shown in Tables I and II. The first column of Table I indicates the characteristic judged. Columns two and three show the figure number and the judgment number for each time period. Column four shows the percent of choice. (Table III follows the same explanation as Table I and Table IV the same explanation as Table II.) Table II shows the number of subjects who reported a decrease or an increase in area, in the broken spaces or gaps, or in curvature of lines. The extreme left hand column shows the judgment greater, wider, etc., while the extreme right hand column shows the judgments narrower, smaller, etc. Columns two, three, four, five, and six show the number of subjects that reported 100 percent of the time or 75 percent of the time in either direction, *i.e.*, either toward a smaller or larger gap, smaller or larger area or more or less curvature. The fourth column shows the number of subjects that reported an equal number of times for increase or decrease in size, area, curvature, etc. The Chance Error was calculated [3] from Bernoulli's formula (2) for Tables I and III.

[3] Percent sigma equals the square root of pq/n. p is the number of 'yes' answers. q is the number of 'no' answers. n is the number of cases.

TABLE I

SUMMATION OF RESULTS WITH BLIND SUBJECTS

Characteristic Judged	Figure Number	Judgment Number	Percent of Choice
The gap in the circle was judged wider than in the first circle seen.	1	1	85*
	1	2	85*
	1	3	95*
	1	4	90*
Area of the circle judged greater than the area of the first circle seen.	1	1	90*
	1	2	90*
	1	3	100*
	1	4	80*
Width of the angle was judged greater than the width of the first angle seen.	2	1	35*
	2	2	40
	2	3	50
	2	4	30*
Length of side line judged longer than the length of the line on the first figure seen.	3	1	75*
	3	2	55
	3	3	25*
	3	4	40
The curve of the arc was judged more curved than the curve of arc in the first figure seen.	3	1	50
	3	2	40
	3	3	30*
	3	4	35*
Length of the baseline of the angle was judged longer than the baseline in the first figure seen.	4	1	35*
	4	2	55
	4	3	35*
	4	4	40
The width of the angle was judged greater than the width of the first angle seen.	4	1	45
	4	2	60
	4	3	65*
	4	4	60
The area of the square was judged greater than the area of the first square seen.	5	1	85*
	5	2	90*
	5	3	95*
	5	4	80*
The figure was judged more nearly square than the first dot figure seen.	5	1	40
	5	2	30*
	5	3	30*
	5	4	20*
The gap in the triangle was judged greater than the gap in the first triangle seen.	6	1	25*
	6	2	50
	6	3	45
	6	4	40
The area of the triangle was judged greater than the area of the first triangle seen.	6	1	45
	6	2	40
	6	3	30*
	6	4	35*

* These figures lie outside Chance Error percent sigma = 11.

TABLE II

A Comparison of the Number of Blind Subjects Reporting Shrinkage in 100 Percent and 75 Percent of Their Judgments with Those Blind Subjects Reporting Expansion in 100 Percent and 75 Percent of Their Judgments

Judgment	100%	75%	50%	75%	100%	Judgment
			Figure 1			
Wider	13	5	2	0	0	Narrower
Larger	13	5	0	0	0	Smaller
			Figure 2			
Greater	2	3	4	6	5	Lesser
			Figure 3			
Longer	3	7	2	6	2	Shorter
More curved	0	5	3	9	3	Less curved
			Figure 4			
Longer	3	5	1	4	7	Shorter
Greater	3	7	4	5	1	Lesser
			Figure 5			
Greater	12	7	0	1	0	Lesser
More nearly square	0	2	3	10	5	Less nearly square
			Figure 6			
Greater	1	5	4	6	4	Lesser
Greater	2	1	6	6	5	Lesser
Total	52	54	29	53	32	

The Chance Error for Table I was found to be eleven. Inspection of Table I shows that in some cases, *i.e.*, Fig. 1 and Fig. 5, there is definite evidence to support the statement that the laws of Prägnanz [4] and Closure [5] function with the blind. The subjects reported the figures to be larger and less nearly perfect, which indicated that the memory-image had become smaller and more nearly perfect.

The percentages for the other figures appear to show a startling amount of inconsistency. This might be explained in the following manner. First, the small number of cases used for this study causes a high Chance Error. Second, observation of the method employed

[4] Law of Prägnanz: The whole field becomes as well articulated as possible (15).

[5] Law of Closure: The segregated wholes tend to become completed or closed forms (15).

in perceiving by blind subjects suggests that the possibility of getting either sharpening or leveling is affected by the figure's position in relation to the ground and to the subject. For example, the fingers of the subject when exploring Fig. 2 seemed to find an imaginary baseline on which the apex of the angle rested. The figure was presented so that the apex of the angle was down, or toward the subject. When the subject reversed the board so that the apex was pointing up or away, this imaginary baseline changed position so that the legs of the angular figure rested upon it.

In the first case where the apex of the angle pointed down, the expected leveling took place, and the angle was reported more acute than the first angle presented. Those subjects, however, who pointed the angle up or away from them instead of getting the expected leveling, reported closure. Of the twenty subjects, nine reversed the position of the board. Of these nine, five always gave reports indicating closure, and four gave reports indicating closure in 50 percent of their judgments. Of the remaining eleven subjects who perceived the figure with the apex down or toward them, five reported leveling and four reported leveling in 75 percent of their judgments. In general, then, it appears that with these blind subjects an obtuse angle with the apex up will tend to elicit reports of closure while the same angle with the apex down will tend to cause the subjects to report leveling.

With relation to Fig. 4, Table I, there seems to be little explanation for the inconsistency of the results. Half of the subjects not only reversed the stimulus board but also explored it while the apex was pointed toward the bottom, top, and right side. Of these ten subjects, six reported the baseline of the figure longer, three reported it shorter, and one gave alternate reports on each judgment. The same subjects reported as follows concerning the change in the width of the angle: four reported an increase in size, four a decrease in size and two gave mixed reports [6] of increase and decrease with each judgment. There is a noticeable difference with the ten subjects who perceived the angle from the position with the apex pointing to the left and who did not change the position of the board. Eight of the ten subjects reported a shortening of the baseline; two reported it longer. Six reported the angle under judgment as larger, two reported it smaller, and two gave alternate reports. On the basis of this comparison it would appear that when the position of the figure is changed there is less indication of closure and sharpening than when the figure is perceived with the baseline down and the apex to the left.

[6] Half of a given subject's judgments were toward closure, etc., and half towards expansion, the judgments being mixed.

The above comparison of the results and an investigation of Table II indicates why the percentages in Table I do not show a definite tendency. It can be seen that the opposing reports of the group of subjects would have a tendency to cancel one another and make the percentages appear to be neutral, whereas, really there are two subgroups in opposition, one showing a trend towards closure, the other showing a trend towards expansion. Table II shows a different picture. For Fig. 1 in Table II there is sufficient evidence to say definitely that with the passage of time there is both a decrease in size of the memory-image, and closure. There is also good evidence of progressive change. For Figs. 2, 3, and 4 there is again evidence of progressive change, but no evidence to support the conclusion that it must always be in one direction. Again the question is raised as to what part perception plays in the direction of change. Figure 5 gives the same indications as Fig. 1, however, there is indication in these results that the direction of change might be either toward contraction or expansion of the memory-image. Figure 6 shows a tendency for the subject to display more indecision. The opposed tendency reports for this figure is twice that of the others. It should be noted, however, the tendency is for expansion rather than shrinkage.

Procedure with Sighted Subjects

The group of sighted subjects was divided into four sub-groups of six subjects each. The first sub-group consisted of subjects seven to twelve years of age, the second from twelve to fifteen years of age, the third from seventeen to nineteen years of age, the fourth from twenty-one to thirty-seven years of age. In each group two were presented the stimulus boards behind the hood in a normal position, the subjects exploring them under the hood; two were presented the boards in a reverse position under the hood; two were shown the exact reproduction of the figures on cards at a distance of five meters, for visual examination. The subjects were treated as nearly as possible as were the blind subjects, with one exception. The sighted subjects were not permitted to change the position of the stimulus boards after they were presented in either the normal or reverse positions. Those subjects who perceived the stimulus visually were subjected to the same conditions as were the other sighted subjects. It was necessary to be sure that these subjects did not think they were being shown the same stimulus at each of the trials. Therefore, the stimulus figures were presented on both white and pale yellow cards in the *abba* order. The cards were presented at a distance of five meters from the subject.

Results of Sighted Subjects and Discussion

In Tables III and IV the results of the experiment with the group of sighted subjects are shown. The Chance Error for Table III was found to be ten. As in Table I there is evidence to support the statement that the laws of Prägnanz and Closure function for Figs. 1 and 5. Again as in Table I the results in Table III indicate considerable inconsistency with the exception of Fig. 6. In Table I, Fig. 6 did not show a definite trend in either direction. In Table III, Fig. 6 shows a definite trend toward expansion.

TABLE III

SUMMATION OF RESULTS OF SIGHTED SUBJECTS

Characteristic Judged	Figure Number	Judgment Number	Percent of Choice
The gap in the circle was judged wider than in the first circle seen.	1	1	58
	1	2	83*
	1	3	88*
	1	4	83*
Area of the circle judged greater than the area of the first circle seen.	1	1	45
	1	2	66*
	1	3	75*
	1	4	66*
Width of the angle was judged greater than the width of the first angle seen.	2	1	50
	2	2	58
	2	3	58
	2	4	66*
Length of side line judged longer than the length of the line on the first figure seen.	3	1	45
	3	2	58
	3	3	37*
	3	4	33*
The curve of the arc was judged more curved than the curve of arc in the first figure seen.	3	1	66*
	3	2	66*
	3	3	71*
	3	4	58
Length of baseline of the angle was judged longer than baseline in the first figure seen.	4	1	42
	4	2	54
	4	3	45
	4	4	58
The width of the angle was judged greater than the width of the first angle seen.	4	1	45
	4	2	29*
	4	3	33*
	4	4	42
The area of the square was judged greater than the area of the first square seen.	5	1	42
	5	2	42
	5	3	33
	5	4	29*
The figure was judged more nearly square than the first dot figure seen.	5	1	33*
	5	2	58
	5	3	37*
	5	4	37*
The gap in the triangle was judged greater than the gap in the first triangle seen.	6	1	13*
	6	2	8*
	6	3	8*
	6	4	8*
The area of the triangle was judged greater than the area of the first triangle seen.	6	1	33*
	6	2	42
	6	3	25*
	6	4	25*

* These figures lie outside the Chance Error percent sigma = 10.

TABLE IV

A Comparison of the Number of Sighted Subjects Reporting Shrinkage in 100 Percent and 75 Percent of Their Judgments with Those Sighted Subjects Reporting Expansion in 100 Percent and 75 Percent of Their Judgments

Judgment	100%	75%	50%	75%	100%	Judgment
			Figure 1			
Wider	13	7	2	0	2	Narrower
Larger	9	4	6	1	4	Smaller
			Figure 2			
Greater	5	6	6	5	2	Lesser
			Figure 3			
Longer	2	4	7	8	3	Shorter
More curved	7	8	3	5	1	Less curved
			Figure 4			
Longer	3	6	6	6	3	Shorter
Greater	3	1	5	11	4	Lesser
			Figure 5			
Greater	1	4	7	6	6	Lesser
More nearly square	3	4	4	8	5	Less nearly square
			Figure 6			
Greater	0	1	1	4	18	Lesser
Greater	0	5	4	6	9	Lesser
Total	46	50	51	60	57	

The inconsistency shown in Table III might be the result of the high Chance Error, as was pointed out concerning the blind subjects in Table I. There is also the possibility that because one-third of the subjects perceived the boards in a different position from the other, *i.e.*, reversed, the same concellation effect took place which was considered in the discussion of the results of the blind subjects. Investigation of Table IV shows that the results of Table III are due to cancellation just as Table II indicates a cancellation effect in Table I. That is, the opposing reports of the subjects have a tendency to cancel one another and make the percentages appear to be neutral.

Again, as in Table II, Table IV shows that there is a progressive change of the memory-image but that it is in either dircetion, *i.e.*, toward a smaller figure, narrower gaps, etc., or toward a larger figure, wider gaps, etc.

It is interesting to note at this point that there are nearly twice as many alternate reports for the sighted group of subjects in the fourth column of Table IV as there are for the blind subjects in the corresponding column of Table II. This is probably due to the greater *naïveté* of the blind group. If this is the case, the more sophisticated sighted group seemingly were not inclined to repeat their judgment. Thorndike (**14**) has reported such an attitude.

A study of the results of the eight subjects who were presented the stimulus visually showed that there was definite evidence that the memory-image did not always show shrinkage. There was only one less judgment indicating expansion, than indicating shrinkage. Although the totals of the tables show, in some cases, progressive change it must be remembered that if they followed the normal curve the majority of judgments would show as mixed. Therefore, these results seem indicative.

General Discussion

The differences between sexes for the blind subjects were as follows: the boys gave more alternate reports than did the girls, the judgments of the girls appeared to point to expansion of image to a greater extent than did those of the boys. The greater number of alternate reports made by the boys may have been the result of an attitude against repeating judgments as was pointed out above. The reports of closure were nearly the same, the girls reporting one more judgment for closure than did the boys.

Comparing the age range groups in the sighted subjects one thing was noticeable. The pre-adolescent group showed much less indecision than any of the other age groups. The early adolescent group gave the highest number of alternate reports, with the mature group giving the next highest number. In all four groups progressive change was shown, but it did not show change going in the direction of shrinkage alone. For the pre-adolescent group, there was a total of 29 judgments showing expansion of the figure as against 32 for shrinkage. In the early adolescent group there were 20 judgments showing shrinkage of the memory-image and 27 showing expansion. The adolescent group gave a total of 21 judgments showing shrinkage of the memory-image and 33 showing expansion. The mature group gave 24 judgments for shrinkage and 26 for expansion. The sighted

group shows the following judgments [7] indicated in Table IV: closure in 100 percent of judgments, 46; 75 percent of judgments, 50; expansion in 100 percent of judgments, 57; 75 percent of judgments, 60. The blind subjects gave judgments,[8] Table II, closure in 100 percent of judgments, 52; 75 percent of judgments, 54; expansion in 100 percent of judgments, 32; 75 percent of judgments, 53. These figures indicate that the blind subjects reported shrinkage and closure more often than did the sighted subjects under comparative conditions.

Because there were only three subjects judged to be below normal intelligence no comparison was made with those judged normal. Examination of the score sheets of these three subjects does not indicate that there is any difference between their reports and the reported judgments of the normal subjects.

The use of the same stimuli throughout the experiment means that any change in judgment is a change in memory-trace. An attempt was made to relegate 'recall' and 'recognition' by the specific instructions that the figures were similar. Wulf (**17**) found it necessary to re-present some of his figures in order to insure the necessary 'recall.'

As is reported by Bean (**4**) study of perception through active touch is apparently much more objective than visual perception. The observer is able to follow the perceptive movements of the hands and fingers with great detail. Bean (**4**) also reports that perception in the blind is very similar to that of the sighted. This was found to be true in this research also when questions concerning perception were asked of the eight subjects who perceived the stimulus visually. The questions were phrased in such a way that the subject could answer 'yes' or 'no,' *i.e.*, "Did you take into consideration the amount of ground outside the figure when making a judgment concerning the area of the figure?"

From the standpoint of the observer Fig. 3 was perhaps the most interesting. The blind subjects who indicated closure, when following the line of the semicircle, invariably went beyond the end of the figure in relief in a direction indicating that they expected the figure to continue as a circle. It usually took several trials for them to satisfy themselves that the figure was incomplete. Those subjects who did not report a more perfect figure (the arc under judgment being reported more curved) showed a tendency while following the curve to run over the figure to the outside and stop movement before the finger reached the end of the figure. The sighted subjects re-

[7] The total number of judgments reported in Table IV is 264, of these 51 are mixed judgments, leaving 213 to show either shrinkage or expansion of the memory-image.

[8] The total number of judgments reported in Table II is 220, of these 29 are mixed judgments, leaving 191 to show either shrinkage or expansion of the memory-image.

acted somewhat differently. They, instead of comparing all parts of the board with other parts, seemed to depend on kinesthetic responses of the hand and arm muscles for their judgments. The exploration of all figures by the blind subjects followed the same general procedure. The fingers of the subject would explore the ground first and then the figure raised in relief. After careful exploration of the figure, the ground would again be explored. When asked to make a judgment the blind subject always explored the relation of the ground to the figure. The sighted subjects working under the hood seldom made this exploration. In most cases the fingers of the blind subjects tried to follow a figure which the subject reported to be that which he remembered. In exploration of Fig. 1 the fingers always went beyond the end of the figure in relief, *i.e.*, beyond the end of the wire into the open space. In determining the area of all figures the fingers often went from the edge of the ground past the figure in relief to a point inside the figure where their memory indicated that the figure should be. The relation of the figure to the amount of ground seemed to be an important part of the judgment.

Wulf and others (**17**, **13**, **9**) report that with the passage of time there is a decrease in the size of the reproductions of the stimulus figure. Hanawalt (**10**) did not find this to be true in all cases. The results of the present study show definitely that there is not always a decrease in the size of the memory-image, and that not all subjects reporting a decrease in size did so 100 percent of the time. This study does not show what factor causes some of the subjects to report expansion of the memory-image. Further experimentation will be necessary to determine the cause of this reaction. It might lie in the relation of the figure to the amount of ground surrounding the figure. This is indicated by the careful study of the ground by the blind subjects. Expansion was noted more frequently for Fig. 6 than for any of the other figures (see all tables). This figure was slightly smaller in proportion to the amount of ground than any of the other figures.

Wulf (**17**) also indicated progressive change in the report of his study. The result of the present study bears out his contention. In the majority of judgments there is evidence of progressive change.

It is the belief of this experimenter that this study was made as completely objective as the general conditions of the study would allow. In no part of the study was either experimenter or subjects concerned with introspective reports. All questions were asked in such a way that one of two possible answers had to be given. This served a double purpose. First, the introspective reports in answers to questions were reduced to a minimum. Second, it avoided the possibility of the subject's making a middle choice. The middle

category was purposely left out and the subjects rather forced to one extreme or the other. Many studies have shown that most subjects tend toward the central choice or that there is a "central tendency" (16). Very few of the subjects expressed any doubts that there was any difference in the figures. In cases where doubt was expressed the part of the instructions were repeated as follows: "Remember, this is an experiment dealing with small differences." This appeared to erase the doubt in the mind of the subject. Often a spontaneous remark, as follows, would be made: "This circle is much larger than the one I first saw." Most of the subjects seemed certain of their judgments. Rhythm and rate of presentation were eliminated completely. Fatigue, pathological conditions, and drugs played a very small part, apparently. The subjects reported that they considered their physical condition normal for themselves, that they were no more fatigued than was usual for them at the time of day tested. Some of the subjects smoked, but no other drug was reported used. As only one race was used for subjects no conclusions can be drawn concerning any racial differences. With the exception of the eight subjects who were given a stimulus visually, the only sense organ involved was that of tactile stimulation of the hand.

Conclusions

It was the purpose of this study to determine whether, with the passage of time, figures do become 'better' and whether the change of the memory-image is 'progressive.' From the results of the present study it can be concluded that figures are apt to become 'better' in the memory-image, but that the direction of change toward shrinkage or expansion of the memory-image cannot be predicted. On the basis of this study it appears that if either occurs it will occur 'progressively.'

The conclusions drawn are not intended to go beyond the results found with the subjects employed in this study. This statement is made advisedly in order to stress the necessity for further studies of this type. The writer fully realizes that the number of cases presented is too small from which to draw definite conclusions. There are, however, certain suggestive questions arising from the study that point the way for further experimentation. First, does the amount of ground surrounding the figure perceived play any part in the expansion or shrinkage of the memory-image? Second, what caused some subjects to report shrinkage and others expansion for the same stimulus? Third, what caused the alternate reports made by some subjects concerning the same stimulus?

(Manuscript received May 28, 1941)

References

1. Allport, G. W., Change and decay in the visual memory image, *Brit. J. Psychol.*, 1931, **21**, 133–149.
2. Arkin, H., and Colton, R., *An outline of statistical methods*, 1938, New York, Barnes and Noble, pp. 270.
3. Baldwin, M. J., and Shaw, W. J., Memory for square size, *Psychol. Rev.*, 1895, **2**, 236–273.
4. Bean, C. H., The blind have optical illusions, *J. exp. Psychol.*, 1938, **22**, 283–289.
5. Blankenship, A. B., Memory span—a review, *Psychol Bull*, 1928, **35**, 1–25.
6. Brown, W., Growth of memory images, *Amer. J. Psychol.*, 1935, **47**, 90–102.
7. Carmichael, L., Hogan, H. P., and Walter, A. A., Experimental study of effect of language on the reproduction of visually perceived form, *J. exp. Psychol.*, 1932, **15**, 73–86.
8. Dashiell, J. F., *Fundamentals of general psychology*, 1937, New York, Houghton Mifflin, pp. 655.
9. Fehrer, E. V., An investigation of learning of visually perceived forms, *Amer. J. Psychol.*, 1935, **47**, 187–221.
10. Hanawalt, N. G., Memory traces for figures in recall and recognition, *Arch. Psychol.*, 1937, **216**, 1–89.
11. Hayes, S. P., My studies in psychology of blindness, 1938, Lansing, Mich., *Amer. Assoc. of Instructors of the Blind.*
12. Irwin, F. W., and Seidenfeld, M. A., The application of the method of comparison to the problem of memory change, *J. exp. Psychol.*, 1937, **21**, 363–381.
13. Perkins, F. T., Symmetry in visual recall, *Amer. J. Psychol.*, 1932, **44**, 473–490.
14. Thorndike, E. L., The refractory period in associative process, *Psychol. Rev.*, 1927, **34**, 234–240.
15. Warren, H. C. (Ed.), *Dictionary of Psychology*, 1934, New York, Houghton Mifflin, pp. 371.
16. Woodworth, R. S., *Experimental Psychology*, 1938, New York, Henry Holt, pp. 889.
17. Wulf, F., Tendencies in figural variation (Trans. by), in Ellis, W. D., *A source book of Gestalt psychology*, 1939, New York, Harcourt Brace, pp. 403.

THE EFFECTS OF PRESSURE IN THE MIDDLE EAR

BY ERNEST GLEN WEVER, CHARLES W. BRAY, AND MERLE LAWRENCE

Princeton University

A change of pressure in the middle ear is a common experience. Upon variation of altitude, as in elevators, mountain climbing, and aerial flight, the pressure difference between the atmosphere and the air of the middle ear cavity is readily noticed. Ordinarily the effects are slight and transient, since in the act of swallowing the Eustachian tube is opened to permit equalization. In some instances, however, when the Eustachian tube is blocked, equalization cannot take place and the pressure difference becomes large and distressing. Also, when the tube is blocked an alteration of pressure may be brought about without change of altitude, through absorption of air in the middle ear cavity.

It is well known that these pressure changes have an effect upon hearing. Ordinary experience, and many clinical observations, show in general a reduction of acuity as a result of the pressure difference. However, only a few experiments have dealt with the problem. Fowler (**2**), with human subjects, varied pressure by means of a cannula in the Eustachian tube and reported a reduction of hearing, especially for low tones, as measured both by air and bone conduction. Kobrak (**5**), with rabbits, introduced pressure into one bulla while observing the tensor reflex on the opposite side, and found moderate reductions for all tones used. Thompson, Howe, and Hughson (**7**), in experiments on cats with the electrical potentials of the ear as the indicator, introduced pressure either into the bulla or the Eustachian tube, and obtained reductions for positive and negative pressures up to 50 mm of mercury.

Other experiments by Kessel (**4**), Fowler (**2**), Pohlman and Kranz (**6**), and Békésy (**1**), in which pressure was introduced into the external meatus, are not exactly comparable. This procedure is the counterpart of the other as concerns the production of tension on the drum, in that positive pressure in the meatus has the same effect as negative pressure in the middle ear, and conversely. However, the effects upon the inner ear do not follow the same order. With positive pressure in the meatus, for example, a positive pressure, reduced by the resistive force of the drum, is communicated to the middle ear cavity and thence to the inner ear. With negative pressure in the middle ear, on the other hand, the same negative pressure is exerted upon the inner ear. In each case, moreover, the effect upon

the inner ear is complicated by displacements of the stapes. Of course, if the inner ear effects were negligible, and only effects upon the drum need be considered, the two procedures would be interchangeable, but the available evidence indicates that such is not the case.

In the present experiments the problem of middle ear pressure was investigated with cats as experimental animals, and the cochlear potentials were employed as an index of auditory action.

Procedure

The middle ear was approached ventrally and a wide exposure of the bulla made by removal of the lymphatic glands in this region, resection of the stylohyoid and digastric muscles, and division of the external carotid artery.

Two holes were drilled in the bulla. One, about 2 mm in diameter, was located laterally near the jugular process of the occipital bone. The other, about 5 mm in diameter, was placed on the mesial side of the bulla at the same level as the first along the sagittal axis. The larger opening provided a view of the round window niche and a portion of its membrane, and made possible the placement of an electrode through the smaller hole onto the membrane.

Both holes were carefully reamed and fitted with rubber stoppers. The electrode wire passed through the smaller stopper, and carried a piece of silver foil on its end. The lengths of wire and foil were adjusted so that on insertion of the stopper in the opening the tip of the foil made contact with the surface of the round window membrane. After this placement the larger opening was closed with a stopper through which was passed the small end of a tapered glass tube, the body of which was then connected by rubber tubing to the pressure apparatus. All connections were made air-tight, and the Eustachian tube was kept closed in a simple but effective way by bearing firmly on its membranous portion with the spatulate end of a rod fixed in a holder.

Pressure was produced by a rubber bulb, and was indicated by a Tycos sphygmomanometer gauge, supplemented by a water manometer for small values.

The animals were maintained under Dial anesthesia. Though surgically suitable, this form of anesthesia has little depressive effect upon the tympanic muscles, and it was found that even slight pressures in the middle ear excite vigorous contractions of these muscles. In most of the experiments, therefore, complicating effects of tympanic muscle reflexes were eliminated by curarization of the animal. Curare was injected through the femoral vein in sufficient amount to stop pinna reflexes and respiratory movements. Artificial respiration was begun at this point and continued for the duration of the experiment.

Pure tone stimuli were delivered from a loudspeaker through a tube opposite the ear, and in some of the experiments from a bone-conduction receiver applied usually to the mastoid process. The cochlear potentials picked up from the round window membrane were amplified and measured with a General Radio Type 736-A wave analyzer operated as a voltmeter. The experiments were carried out on 14 ears.

Results

An increase of pressure in the middle ear cavity causes a marked change in the cochlear responses. Figure 1 shows results for a number of stimulus frequencies. The intensity of each tone was adjusted to give a response of 100 microvolts for the normal condition, shown as zero pressure on the graph, and was then maintained at that value. The pressure was altered by steps usually of 5 or 10 mm of mercury until 50 mm was reached.

It should be mentioned here that after any change of pressure a period of adjustment ensues before the responses reach a stable value. The length of this period varies greatly in different animals from only a second or so to as much as two minutes. Usually at the introduction of pressure the responses fall greatly, and then rise, at first rapidly and then more slowly, until the final level is reached. Similarly, at the release of pressure the responses do not attain their full value at once, but rise gradually. However, the course of these

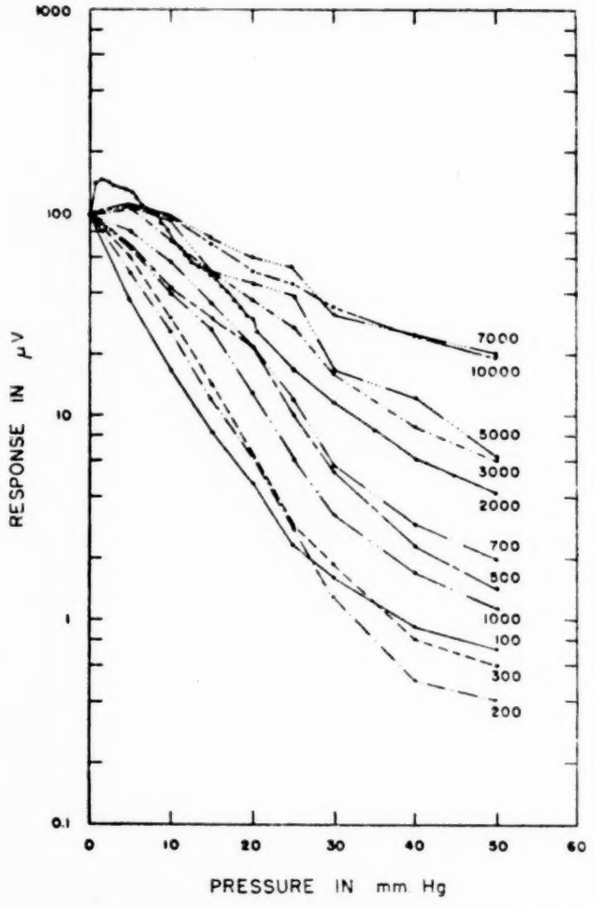


FIG. 1. The effect of middle ear pressure upon responses to various tones. Frequencies are indicated by the numbers at the right ends of the curves.

adjustments is not always so simple as this, and sometimes with certain tones the responses 'overshoot,' or the curve of change reverses its direction several times. No certain cause can be assigned for this feature; its character suggests a degree of pliability and alteration of mechanical efficiency in the system under the influence of pressure. In the results presented here, care was taken to allow sufficient time for stabilization after any pressure change.

A further observation is that the cochlear responses are highly susceptible to respiratory changes. If ventilation is reduced their magnitude drops sharply, and is then gradually restored if adequate

ventilation is resumed. No complete study of the relation to respiration has been carried out. In this study an attempt was made to keep the respiratory level constant for any set of observations. When this was done the responses remained stable for hours.

As Fig. 1 shows, the effects of pressure vary somewhat for different tones. For certain ones the first effect is a slight improvement of response. Larger pressures, however, produce marked reductions.

The reduction effect is greatest for the lower tones, and becomes generally less as the frequency is raised. For example, a pressure of 50 mm of Hg caused a drop in the response to 100~ to less than 1 percent of its initial value, a loss of over 40 db, while for 10,000~ the drop was only to one-fifth the initial value, or a loss of 14 db. The

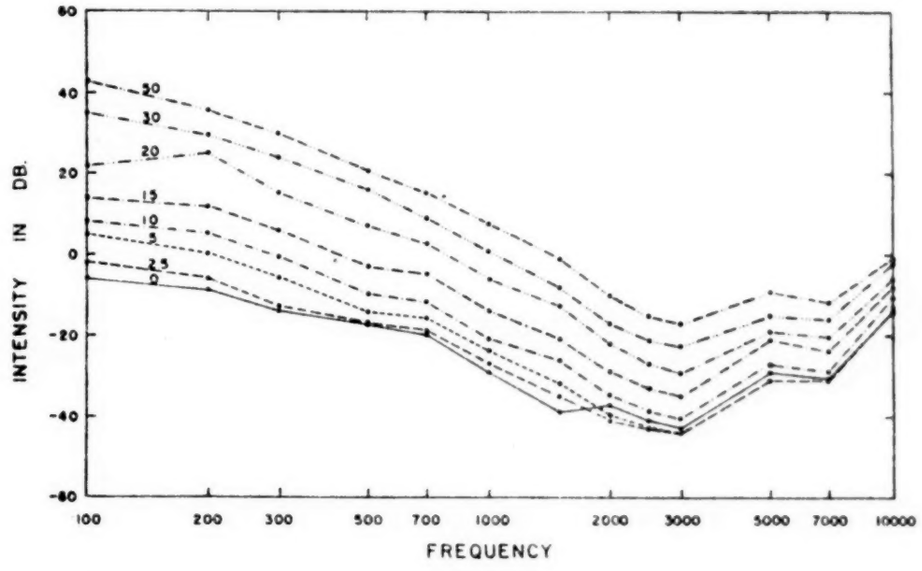


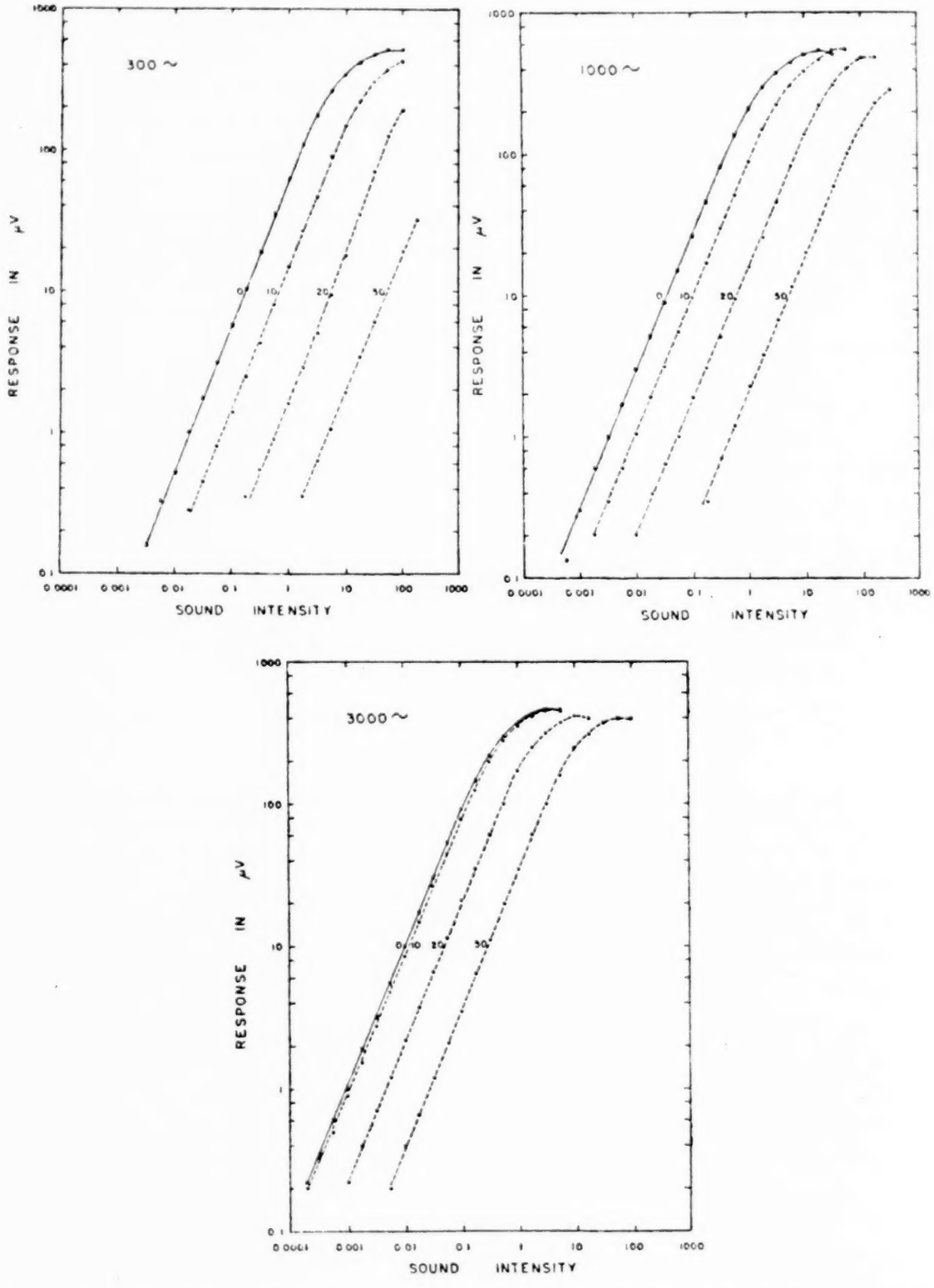
FIG. 2. Equal-response curves for different degrees of pressure. For each curve the pressure is constant, and is indicated in mm of Hg by the number above the curve.

variation is not exactly in order of frequency, however. There are several inversions of this order, and some curves cross in their courses so that the order varies somewhat for different degrees of pressure.

The relation to frequency and to the sensitivity function of the ear is best shown in a different form of plot, like that of Fig. 2. This figure presents equal-response curves, which show the stimulus intensity required at each of several frequencies to produce a standard response of 10 μv. The different curves represent different conditions of pressure as given in mm of Hg by the number above the curve. The curve marked 'O' is the normal sensitivity curve. The ordinate expresses intensity in db above and below a reference level of 1 dyne per sq. cm.

These curves show plainly the effect of pressure upon sensitivity.

For the larger pressures the intensity required for the standard response is greater. This effect is more prominant for the low tones, and thus the curves diverge toward the left.



FIGS. 3–5. Intensity functions for 300, 1000, and 3000~. Each function is for a particular degree of pressure as shown by the number near the middle of the curve.

In addition to the effect mentioned, the curves undergo a particular alteration of form in the region between 2000 and 7000~. Here the curves for the two smallest pressures run below the normal curve, and indicate an improvement in sensitivity. The effect is a smoothing of the curve in this region, and the altered form is maintained at higher levels of pressure where sensitivity becomes impaired.

These features, and particularly the changes observed in the most sensitive region, vary in detail among different animals, but are sufficiently general to suggest that at least two factors are involved in the action of pressure, one that accounts for the progressive relation to frequency and another that is possibly a change of resonance characteristics in the region of greatest sensitivity.

The next three figures reveal the functional relation to sound intensity. Curves are given for representative tones of 300, 1000, and 3000~. Here each curve shows the variation in magnitude of response with intensity of stimulation for each of four conditions. The curve on the left, marked 'O,' is the normal intensity function. The remaining curves represent pressures of 10, 20, and 50 mm of Hg as indicated.

The effect of pressure is to displace the functions to the right along the intensity axis without causing any notable change in their form. The curves remain straight at low levels of response and bend or 'overload' at high levels as normally. For some of the curves, as for 1000~ at 50 mm and 300~ at 20 and 50 mm, it was not possible to deliver sufficient stimulus intensity to trace out the uppermost portions.

Though the general form of the curves is constant, there is often an observable difference in the response level at which overloading occurs. Under conditions of pressure the curves may bend at slightly different levels. For example, in Fig. 5 the maximum of the curve for O pressure is about 460 μv., while for 50 mm pressure it is about 400 μv., a decrease of 1.2 db. For the other functions the differences are still smaller, and are disclosed only by careful superposition of the curves. Less often, the functions show an increase in the maximum in the presence of pressure. These effects are small, and do not exceed the variations frequently encountered in the maximum value of the responses under normal conditions.

Locus of the Effects of Pressure

Further experiments were carried out to ascertain the locus of the pressure effects. The effects are not dependent upon any active process, such as the contraction of tympanic muscles. Though, as

already mentioned, reflex action of these muscles is excited by pressure when the animal is under simple anesthesia, these effects of pressure are incidental. They disappear after muscular action has been stopped by curarization and death, whereas the principal effects persist.

Figure 6 shows results for a tone of 1000~ obtained under three conditions. The intensity was constant for any one curve. For the curve marked 'normal' the animal was under simple Dial anesthesia,

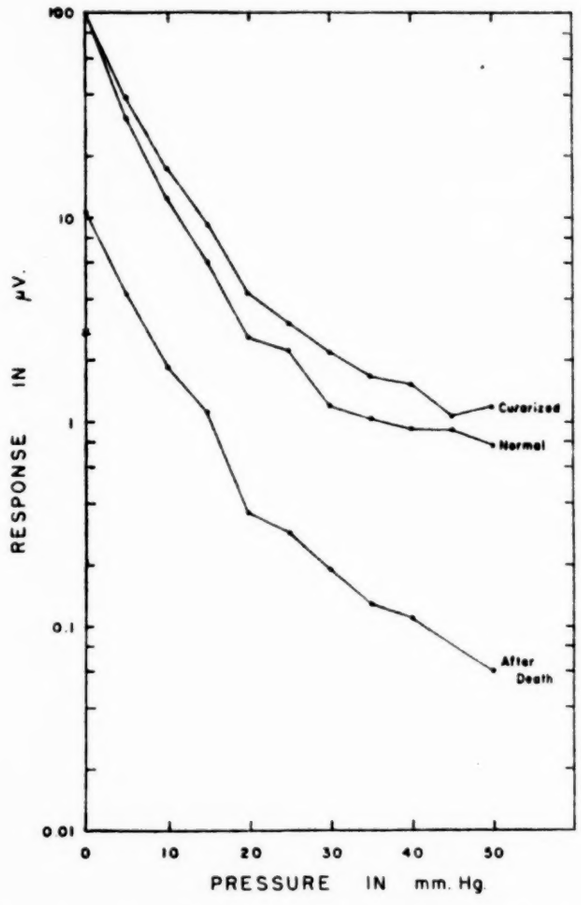


FIG. 6. Pressure curves for 1000~ under three conditions as indicated.

and reflexes were easily elicited. The curve marked 'curarized' represents observations made about 3 hours later, after curarization. The difference between the two curves, which we may attribute to action of the middle ear muscles before curarization, is only a minor variation of form, and the general character of the pressure function remains.

The third curve of this figure was obtained at the end of the experiment when artificial respiration was discontinued and the animal was allowed to die. The responses suffered a rapid decline, and then reached a stage of moderate loss, at which time the observations

on the effect of pressure were made. Due to the loss of sensitivity the initial response could not be obtained at the former level, but the pressure curve has much the same form as before. This is true despite the fact that the amount of drop with increasing pressure is somewhat exaggerated, as the readings were taken in order up to 50 mm and the death loss therefore is superimposed. The cross at 2.7 μv. shows the response obtained for zero pressure at the end of this series of observations, and the difference from the initial value makes it possible to estimate the effect of death itself on the curve.

Further readings for the same tone at a somewhat higher intensity were made after an hour had elapsed, by which time the death function had reached a stage of very slow decline (9). Also, the

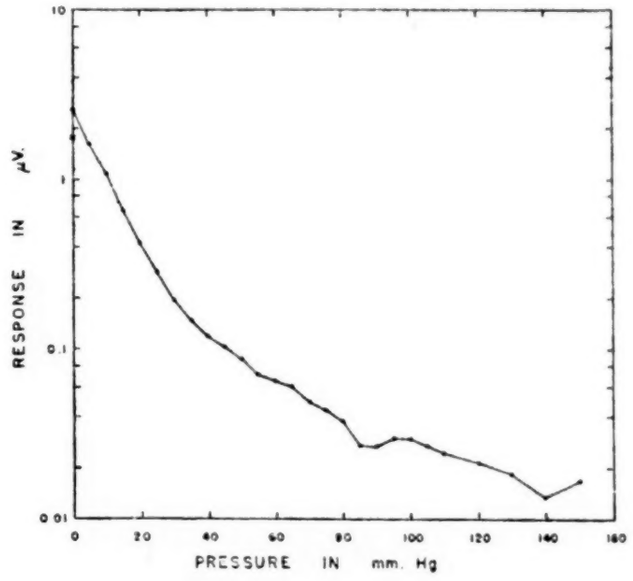


FIG. 7. Effects of pressure 1 hour after death. The drum was ruptured when the pressure exceeded 150 mm of Hg, whereupon zero pressure was restored and the response rose to the value indicated by *x* on the zero line.

pressure was raised until the drum was broken. The results appear in Fig. 7. The initial level of the responses was of course rather low at this time, but since the conditions of observation were particularly favorable due to reduction of the physiological background, sensible readings were obtained almost to a hundredth of a microvolt. The drum broke at 150 mm of Hg, whereupon the response rose almost to its initial value, as shown by the cross at 1.8 μv. on the zero pressure line. Again the difference from the initial value represents the degree to which the curve of loss is accentuated by death changes.

The ear just referred to withstood pressures up to 150 mm of Hg before rupture of the drum. This maximum varies widely in different animals. In two the drums were particularly fragile, and broke at 50 mm. The average probably lies around 80 mm. When

the drum ruptures under gradually augmented pressure no obvious perforation appears. The radial and circular fibers are parted usually at slightly different places and the fiber layers separate, so that air leaks through slowly. The locus of the damage can be made out only on careful scrutiny by alteration of the 'light reflex' of the drum.

In two animals the pressure was introduced into one bulla while observations were made of cochlear potentials from the other ear. It was found that under simple anesthesia, when tympanic muscle

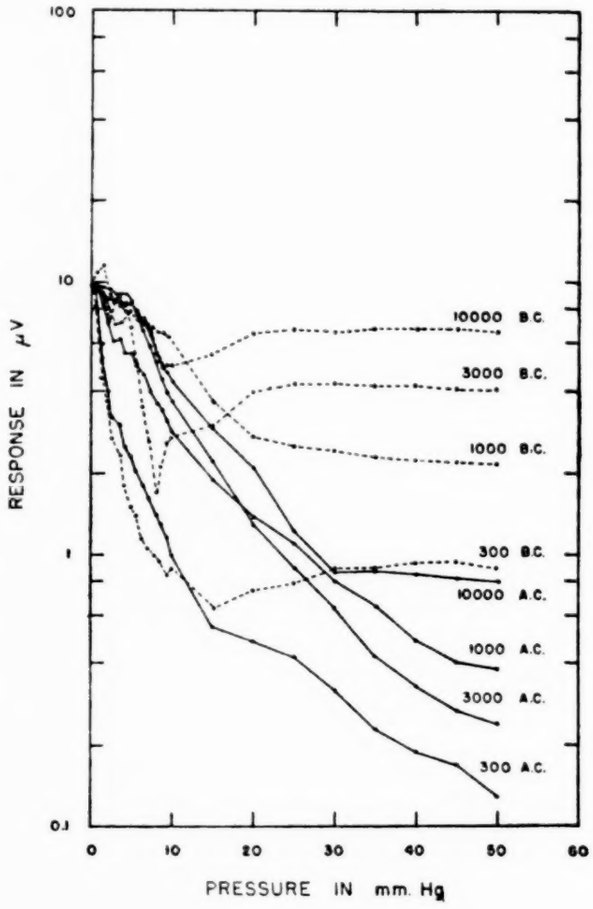


FIG. 8. Pressure curves for four tones presented by both air and bone conduction. The label to the right of each curve shows the frequency and mode of stimulation.

reflexes were present as proved by observation of impulses from the tensor tympani muscle during stimulation of the pinna, the application of pressure to the opposite bulla caused a noticeable change in cochlear responses. However, when the animal was curarized to the point where respiration ceased and striped muscle reflexes could no longer be elicited, the application of pressure to the opposite ear had no effect on cochlear responses. These tests are of value in proving the effectiveness of curarization for the exclusion of tympanic muscle reflexes, and they also eliminate from consideration any pressure

effects transmitted across the head through perilymphatic and cerebrospinal fluids.

In some of the experiments the stimuli were applied by both air and bone conduction. Figure 8 shows results for four frequencies, 300, 1000, 3000, and 10,000~. Each stimulus was adjusted to give a normal response of 10 μv., and then was maintained at that value. The solid lines represent results for air conduction, and the dashed lines for bone conduction.

The bone conduction curves are markedly different from the corresponding air conduction curves. They show large drops at first and then level off, most often after a sharp rise. The air conduction curves, on the contrary, continue their fall even for the highest pressures.

As it is well established that the late inner ear processes are the same for aerial and bone-conducted sounds, it follows that the differences between the effects of pressure shown by the two forms of stimulation are to be sought in the conductive processes. Air conduction is of course primarily dependent upon the drum and ossicular system, while bone conduction occurs without these structures but is affected by them when they are present. Both forms of conduction depend upon fluid pathways in the inner ear, but probably in effectively different ways.

To exclude the action of the peripheral apparatus for bone conduction it is necessary to break the ossicular chain. This operation was carried out through a 5 mm hole drilled in the bulla anterior to the usual two. This hole was located so that after removal of the bony septum of the middle ear cavity the incudo-stapedial articulation could be seen. This articulation was severed. Then, to prevent reestablishemnt of contact between incus and stapes, which ordinarily will occur through the tension of ligaments and muscles, a small portion of the long process of the incus (the lenticular portion) was broken off, and the tendon of the tensor tympani muscle was cut. After the operation the opening in the bulla was closed with a rubber stopper.

This operation is difficult both on account of the restricted field and the necessity of avoiding any undue stresses upon the drum and inner ear, and was carried out with complete success in but one of two attempts. The results are given in Figs. 9 and 10.

Figure 9 shows the effects of a pressure of 20 mm of Hg under three conditions: by air and bone conduction with the ossicular chain intact, and by bone conduction with the ossicular chain broken. The curves are equal-response curves, and each is plotted with respect to a base line for zero pressure under the corresponding condition. Each therefore shows in db the change of stimulus intensity

necessary to restore the response to the standard value of 1 μv. when 20 mm of pressure is introduced.

The uppermost curve, for air conduction, exhibits somewhat more uniformity throughout the frequency scale than often is found. The low tones show only moderately greater effects. The curve for bone conduction with intact middle ear approaches the air conduction curve in the region from 200 to 1000~, and then falls away for

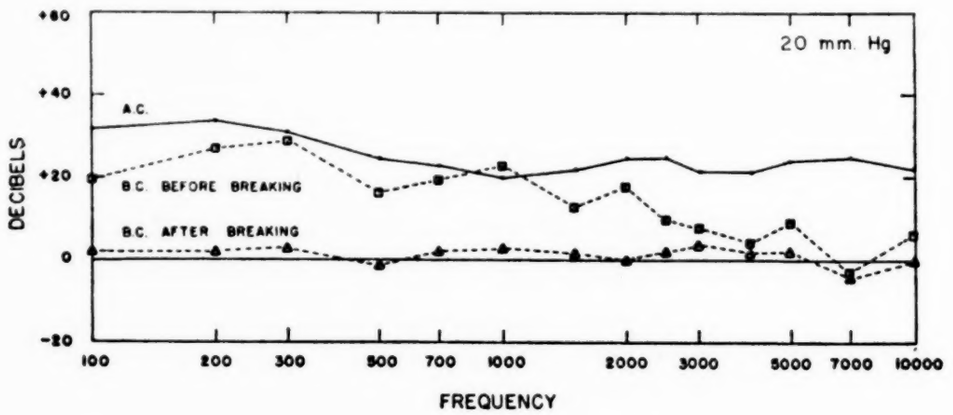


FIG. 9. Pressure effects for 20 mm of Hg before and after breaking the ossicular chain. The two upper curves show results by air and bone conduction before breaking, and the lower curve gives results by bone conduction after breaking the incudo-stapedial joint.

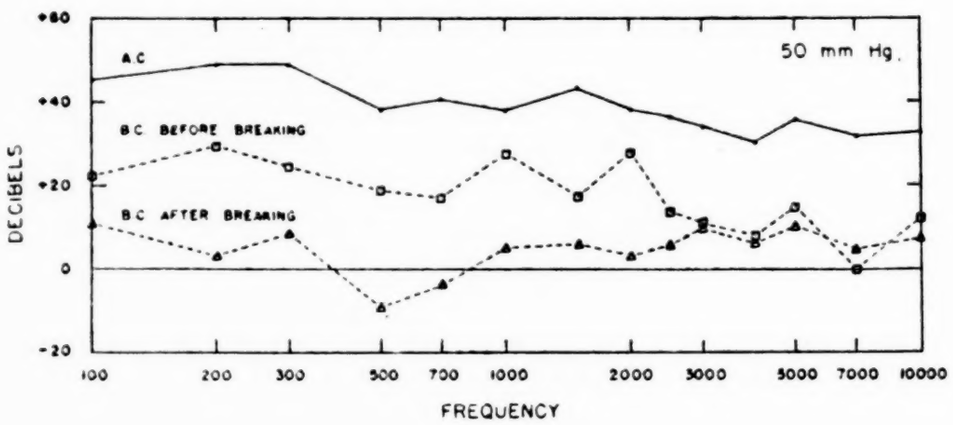


FIG. 10. Pressure effects for 50 mm of Hg before and after breaking the ossicular chain.

the higher frequencies. The relation between these two functions will vary considerably with the level of pressure, as Fig. 8 has shown.

Contrasted with these curves is the one for bone conduction after breaking the ossicular chain. Only slight effects remain, which nowhere exceed 4 db.

Figure 10 shows similarly the effects of 50 mm of pressure. The effect for air conduction is considerably enhanced, but that for bone conduction with normal middle ear is altered but little. The curve for bone conduction after breaking the ossicular chain shows larger

effects than before, but these effects still are small with respect to those for air conduction.

It follows from these observations that middle ear pressure has its principal action upon the drum. The effect mainly is greatly to increase its stiffness.

The increase in the stiffness of the drum probably accounts for the greater effect of pressure on the transmission of low tones. A simple membrane, whose tension is large with respect to the mass, exhibits a form of transmission which varies with frequency. For the range well below its resonance point it has a progressively greater degree of attenuation for low tones. The action of the eardrum is complicated by the loading of the ossicular chain, and the principles which govern the action of a free membrane do not apply in any simple manner. However, it is likely that under the influence of middle ear pressure the operation of the drum approaches more and more closely that of a stiffness-controlled membrane.

The suggestion has already been made that pressure alters the point of resonance of the transmission apparatus. It is probable that this change, like that just discussed, is due mainly to increased stiffness of the drum, though the effective loading also may be enhanced in some degree.

While the principal effect of pressure is located in the apparatus of the middle ear, the evidence shows some effect upon the inner ear itself. As just indicated, when the ossicular chain is broken the larger pressures produce noticeable changes in the responses. These effects may be due either to alterations of transmission in the inner ear or to changes in the sensory cells of the organ of Corti, or both. The evidence of Figs. 3–5, which shows little alteration in the form of the intensity function by pressure, suggests that the effect upon the sensory cells is of negligible magnitude. The most likely cause of altered transmission within the inner ear is the increased tension of oval and round window membranes.

These inner ear effects naturally are present when the ossicular chain is intact, though then they are overshadowed by the drum effect. There is perhaps a further effect upon the inner ear through the lateral displacement of the drum and the communication of this movement to the perilymph. This displacement effect, however, is probably small. Hughson and Crowe (**3**) observed no regular changes of electrical responses on raising the intralabyrinthine pressure, and the writers (**8**) failed to find any significant variations on small displacements of the stapes.

Summary

An increase in the air pressure of the middle ear cavity has a marked effect upon the electrical responses of the cochlea. For certain tones a small degree of pressure produces a slight improvement, but large pressures always cause a reduction of the responses. Generally speaking, low tones are affected more than high tones, but the relation is not exactly in order of frequency.

The introduction of pressure has no essential effect upon the form of the intensity function, for the reduction in sensitivity may be almost wholly compensated for by an increase of stimulus intensity.

Effects are found for bone conduction as well as for air conduction, but the amount of effect differs greatly for these two forms of stimulation. When the ossicular chain is broken and bone conduction is used the effects of pressure are comparatively small. The conclusion is reached that middle ear pressure has its principal effect upon the eardrum, though minor effects evidently arise also in its action upon parts of the inner ear.

(Manuscript received May 28, 1941)

References

1. Békésy, G. v., Zur Theorie des Hörens, *Phys. Z.*, 1929, **30**, 115–125.
2. Fowler, E. P., Drum tension and middle ear air pressures; their determination, significance and effect upon the hearing, *Ann. Otol., etc., St. Louis*, 1920, **29**, 688–694.
3. Hughson, W., and Crowe, S. J., Experimental investigation of the physiology of the ear, *Acta oto-laryngol.*, 1933, **18**, 291–339.
4. Kessel, J., Ueber das Hören von Tönen und Geräuschen, *Arch. Ohrenhk.*, 1882, **18**, 136–151.
5. Kobrak, H., Influence of the middle ear on labyrinthine pressure, *Arch. Otolaryngol.*, 1935, **21**, 547–560.
6. Pohlman, A. G., and Kranz, F. W., The effect of pressure changes in the external auditory canal on acuity of hearing, *Ann. Otol., etc., St. Louis*, 1923, **32**, 545–553.
7. Thompson, E., Howe, H. A., and Hughson, W., Middle ear pressure and auditory acuity, *Amer. J. Physiol.*, 1934, **110**, 312–319.
8. Wever, E. G., Bray, C. W., and Lawrence, M., The origin of combination tones, *J. exp. Psychol.*, 1940, **27**, 217–226.
9. Wever, E. G., Bray, C. W., and Lawrence, M., The nature of cochlear activity after death, *Ann. Otol., etc., St. Louis*, 1941, **50**, 317–329.

THE EFFECT OF VARYING CONDITIONS OF REINFORCEMENT UPON A SIMPLE RUNNING RESPONSE [1]

BY FRANK W. FINGER

Brown University

Introduction

Most of the theories of the learning process, and particularly those attempting a relatively quantitative approach to the problem, have assumed that the strength of the connection between the stimulus pattern and the acquired response is proportional to the amount of reinforcement that has accompanied the S-R sequence. During acquisition the strength of the learned correlation increases with repeated reinforcement; this stronger connection is evidenced by a faster or larger or more accurate response and is reflected by greater subsequent resistance to unlearning. During extinction the process is reversed, and the progressively more feeble response is indicative of the gradual weakening of the bond. It might seem obvious that every reinforced response should produce an increment and each non-reinforcement a decrement of learning.

Within the past few years, however, certain experiments of Humphreys' have cast doubt on the validity of such an assumption (**5**, **6**). He has found, in conditioning the human eyelid and galvanic skin responses, that the omission of reinforcement in 50 percent of the acquisition trials does not always decrease the strength of the final response. Further, he has demonstrated that the subjects trained under this condition of 50 percent random reinforcement resist extinction more than do those of the 100 percent reinforcement condition, at least in the early stages of the extinction series. These data indicate that the temporal pattern, as well as the absolute amount of reinforcement, has an influence upon the changes that take place in the response of the organism. To account for these results Humphreys has postulated an hypothesis of 'expectancy,' suggesting that the effect of reinforcement and non-reinforcement may be understood in terms of "the subject's expectations of the stimuli which are to appear" (**5**, p. 150).

These experiments, which seem to contravert the very nature of accepted learning principles, immediately raise several questions.

[1] Part of the research program reported was originally planned as a collaboration with Dr. C. N. Cofer. The apparatus used in the experiment was constructed by Dr. F. A. Mote.

How widely may these findings be applied? Can like results be reproduced readily in subhuman organisms? Are similar data obtained in other types of learning situation? Do all 'measures' of learning corroborate this demonstrated effect of random reinforcement? Can the results be fitted into other learning theories and squared with a more detailed quantitative approach? May not the development of the 'expectancies' themselves be explicable in terms of excitation and inhibition or some more fundamental concepts?

The purpose of the research here reported was two-fold: (1) to widen the scope of the problem—to another organism (and to one whose response is not facilitated by the verbal capacity), to another type of learning situation, to further measures of acquisition and extinction; and (2) to ascertain whether or not the results can be included in a more comprehensive and quantitative hypothesis of the learning process. In this investigation a so-called operant or instrumental response of the white rat was utilized, requiring the building up of a simple locomotor habit. Reinforcement by food was given in 4 different patterns of either 100 percent or 50 percent-random frequency. Latency of response, number of trials required for extinction, and amount of spontaneous recovery after varying intervals were used as indicators of the extent of learning. This particular learning situation was chosen because numerous aspects of it have already been investigated and a quantitative hypothesis has been evolved to integrate the material (*cf.* esp. Graham and Gagné, 3). It seemed highly desirable to be able to correlate the results of the random reinforcement study with other data obtained under closely comparable conditions. It will be shown that while random reinforcement in this situation leads to results somewhat resembling Humphreys', the application of an expectancy hypothesis here would be premature, if not superfluous.

Apparatus and General Procedure

The apparatus and general experimental procedure have been described in detail elsewhere (3, p. 256). They were adopted with only minor modifications. The apparatus consisted essentially of a simple elevated runway, $3' \times 1\frac{1}{4}''$, at either end of which was a small platform. On each platform could be placed a small box ($5\frac{1}{2}'' \times 6'' \times 7''$), open in front and covered on top with wire mesh. These 2 boxes were interchangeable, and were used alternately as starting box and food box. Between the end of the runway and each platform was a wall-and-sliding-door arrangement so constructed that the rat could be released or detained in the box according to the experimental schedule. The whole apparatus was contained within a wooden frame, covered with cheesecloth. The only illumination came from a 25-watt lamp suspended 12 inches above the runway and 8 inches in front of the food box.

The situation required that the rat respond to the opening of the door in front of the starting box, by progressing down the runway to the food box. A stop watch was started when the door was opened and stopped when the body of the rat passed a mark 4 inches down the runway. This interval, recorded to the nearest $\frac{1}{5}$ second, was called the latent period of the response.[2]

[2] Whenever the rat retraced on the runway to the starting box, the total of all time spent in the starting box or between it and the 4" mark was taken as the latency for the trial.

This latent period was adopted as an immediate indication of the strength of the learned connection, the assumption being made that a shorter latency represents a stronger S-R connection. When the animal had entered the food box the door at this end of the runway was closed and a second watch started. After about 40 seconds the experimenter began preparing the apparatus for the next trial, interchanging the boxes so that the original food box (with the rat still inside) became the starting box. After 60 seconds in this box the rat was thus in the starting position for the next trial. The experimenter then started the next trial by opening the starting door, after a total of 75 seconds had elapsed since the previous entrance into the food box. This basic routine was followed during the acquisition series in 4 of the 5 groups of animals (see below).

The first extinction trial was begun 75 seconds after the last acquisition trial. The only difference in procedure between acquisition and extinction was the omission of reinforcement in the latter instance; the inter-trial interval of 75 seconds was still maintained, measured from the closing of the food box door to the opening of the starting box door for the succeeding trial. The extinction series was continued until that trial during which the rat did not leave the starting box within 3 minutes. This was arbitrarily taken as the point of 'complete' extinction. After the 3 minutes of non-response the door was closed for 4½, 8, or 17 minutes to allow spontaneous recovery to take place. It was then reopened and the latency of any response measured (5 minute limit). Since an animal was tested at only one spontaneous recovery interval, the number of cases at each interval is relatively small, and therefore of limited significance.

Reinforcement during acquisition was given in the form of very small pieces (about 0.04 g.) of Purina dog chow, one of which was placed at the back of the food box before each reinforced trial was begun. A rat consumed no more than 0.7 grams during the course of acquisition, and as Graham and Gagné have pointed out (3), this is not sufficient to affect the level of the hunger drive appreciably.

The experimental session was preceded by 4 sessions of habituation-training. The first of these sessions was conducted with the animal 24-hours hungry. For 10 minutes the rat was allowed to run about on a practice runway (8′ × 1″) but was never given food there. Following this the rat was placed on a small platform for about one minute and was then fed for 20 minutes in one of the experimental boxes in the apparatus. Two similar sessions followed at 24-hour intervals. The fourth habituation session began with the usual 10 minutes on the practice runway, followed by 15 minutes of feeding. During the first part of this feeding period the rat was given one piece of food at a time, to insure his picking up and eating the food promptly during the experimental session, which followed 24 hours later.

The Animals.—The animals were male albino rats from the laboratory colony. Prior to this experiment they had had no training of any kind. Ninety-two animals were given the preliminary training, but 31 were discarded because of failure to respond during the acquisition series or because of some technical error on the part of the experimenter. Five groups of 13 rats each were run under 4 different conditions of reinforcement. Groups I, II, and III were matched approximately with regard to litter and age, being drawn from 4 litter-groups and averaging 115 days of age on the experimental day. Groups IIa and IV were added later, and were similarly matched to each other, representing 3 littler-groups and averaging 78 days.

The Groups.—The 5 groups differed in treatment only with respect to the number of acquisition trials, the number of acquisition trials which were reinforced, and the spacing of the acquisition trials. Extinction and spontaneous recovery proceeded according to a common routine in all groups.

Group I—100 percent reinforcement. The animals of this group were given 16 acquisition trials, all 16 of which were reinforced. The inter-trial interval was 75 seconds.

Group II—50 percent reinforcement. Sixteen acquisition trials were given, 8 of which were reinforced. Reinforcement was given in a pre-determined order, on trials 1, 2, 4, 6, 9, 11, 12, and 15. The inter-trial interval was 75 seconds.

Group IIa—50 percent reinforcement. The treatment of these animals was identical with that of Group II. Four animals run in Group II were also included in the data of Group IIa. This group was added to permit comparison with the results of Group IV, with which it was matched in litter and age.

Group III—100 percent reinforcement. Eight acquisition trials were given, all 8 of which were reinforced. The inter-trial interval was 75 seconds.

Group IV—100 percent reinforcement-spaced. Eight reinforced acquisition trials were

given, spaced to correspond temporally to the reinforced trials of Groups II and IIa. The inter-trial intervals thus varied between 75 and 255 seconds.[3]

For convenience of designation we may combine the number of trials and the amount of reinforcement into a fraction, the denominator of which represents the total number of acquisition trials and the numerator the number of reinforced trials. The factor of spacing may be indicated by the letter S. The groups are then represented thus:

Group I—16/16
Group II—8/16
Group IIa—8/16
Group III—8/8
Group IV—8/8-S.

It can be seen that the 50 percent reinforcement groups (II and IIa) are matched by Group I with respect to total number of acquisition trials, by Group III with respect to number of reinforcements, and by Group IV with respect to the temporal spacing of the reinforcements.

Results

Groups I, II, III

Acquisition.—Figure 1 presents the trend of acquisition for Groups I, II, and III. It may be seen that on the first trial (that is, before any reinforcement had been experienced in the situation) the average response occurred in about 100 seconds or 2.00 log units.[4] The groups are not significantly different in response level at this point; the *t*-value of the difference between Groups I and II is only 0.34. Log latent period of Group I (16/16) drops very rapidly on the next 3 trials, rises briefly, and then continues to decrease to about 0.50 log units. Group II (8/16) decreases until the third trial (which is not reinforced), but there is little further improvement until after the 10th trial. The final acquisition level of this 50 percent reinforced group never equals that of the 16/16 group. A comparison of the last 4 acquisition and first extinction trials of the 2 groups demonstrates that this difference in final acquisition level is reliable, with a *t*-value of 2.8, indicating significance beyond the 1 percent level. The curve for Group III (8/8) follows quite closely the first 8 trials of Group I (16/16). Its final level is significantly higher than that of Group I ($t = 2.4$), but approximately the same as that of the 50 percent reinforced group ($t = 0.43$). In other words, those 2 groups having had the same amount of reinforcement (II and III) are much more alike at the end of acquisition than are the two 100 percent reinforcement groups (I and III) and the 2 groups with the equal number of trials (I and II).

[3] The approximate average time elapsing between the opening of the starting box door and the closing of the food box door was 15 seconds. Adding to this the usual 75-second inter-trial interval, an extra 90-second period in the food box was arbitrarily assigned to represent each non-reinforced trial of Groups II and IIa.

[4] The transformation of latent times into logarithmic units was done in order to correct for the usual skewness of distribution of such measures.

Extinction.—The curves for the first 10 trials of extinction are shown in the left half of Fig. 2. The first 4 points of each curve are based on 13 cases each; later trial averages represent fewer data, because of the dropping out of rats less resistant to extinction. For these first 4 trials the rate of extinction is much more rapid in the 16/16 group than in the 50 percent group, and somewhat more rapid than in the 8/8 group. On the first trial of the extinction series

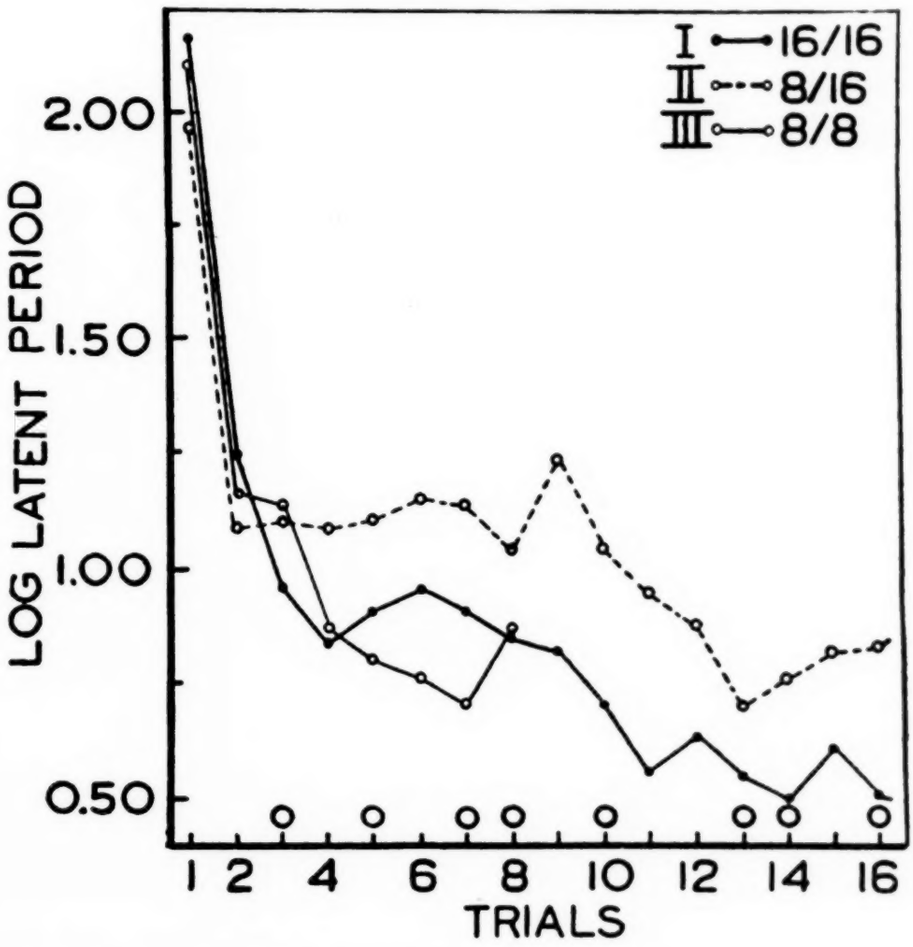


FIG. 1. Average latencies during acquisition series of Groups I, II, and III. The ciphers above the abscissa indicate those trials which were not reinforced in Group II. $N = 13$.

Group I (16/16) is responding 0.42 log units faster than is Group II (8/16), but on the 4th trial only 0.02 log unit separates them. The latency of Group II has increased only 0.19 log units in this interval, as compared to an increase of 0.59 log units for Group I. Beyond this trial the relationships among the 3 curves are rather variable, due in part, perhaps, to the changing number of cases contributing to the points. It is clear, however, that the animals that had the shortest latency at the beginning of extinction are characterized by the most rapid latency increase when reinforcement is completely withdrawn.

A similar relationship holds when we examine the number of trials required to bring extinction to the criterion of 3 minutes of non-response. In Table 1 we see that the 16/16 group (I) required an average of 15.2 trials to reach 'complete' extinction. Group II, having received 50 percent as much reinforcement, distributed randomly over the 16 acquisition trials, needed 27.1 extinction trials to reach the same criterion. This difference of 11.9 trials is significant between the 1 percent and 2 percent levels. Group III (8/8) ran

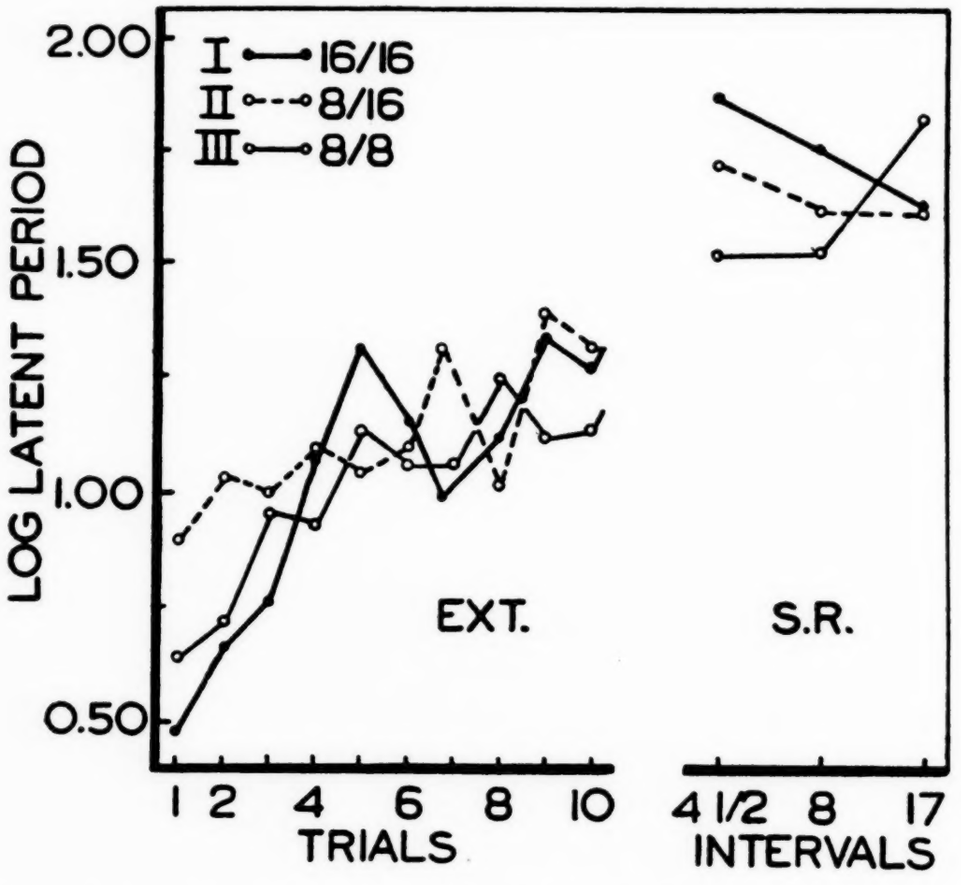


FIG. 2. Average latencies during the first 10 trials of extinction, and during spontaneous recovery, for Groups I, II, and III. $N = 13$ for trials 1–4 of extinction, and is variable thereafter. $N = 3$–5 for each point of spontaneous recovery. See text.

for 23.8 extinction trials—8.6 trials more than Group I and 3.3 less than Group II. The *t*-test indicates that there are 57 chances in 100 that the number of extinction trials for the rats of Groups II and III belong to the same distribution. However, the extinction difference between Groups I (16/16) and III (8/8) is significant at the 8 percent level. These relationships are confirmed when the medians of total extinction trials for each group are compared. The median rat of Group I (16/16) required 13 trials, of Group II (8/16) 21 trials, and of Group III (8/8) 23 trials to reach extinction. Although the final level of acquisition was lowest (fastest response) for

those animals receiving 16 reinforcements, it is quite apparent that this condition of learning also led to more rapid subsequent extinction than did the conditions allowing only 8 reinforcements.

Spontaneous Recovery.—The right half of Fig. 2 represents the course of spontaneous recovery for the 3 groups. The break in the abscissa represents the time elapsing between the tenth trial of extinction and the beginning of the spontaneous recovery period. This varied from rat to rat, depending on the length of time required to

TABLE 1

AVERAGE NUMBER OF EXTINCTION TRIALS REQUIRED TO REACH THE CRITERION OF 3-MINUTE NON-RESPONSE

Group	Condition	*N*	Trials
I	16/16	13	15.2
II	8/16	13	27.1
III	8/8	13	23.8
	Difference	*t*	*P*
II–I	11.9	2.5	<2%
II–III	3.3	0.58	57%
III–I	8.6	1.87	8%

reach extinction. Since the criterion of complete extinction is at the point of 3-minute non-response, the hypothetical last point of the extinction series may be thought of as 2.26 log units. Each point in the recovery curves represents the latent time of response for 3 to 5 animals after the indicated spontaneous recovery interval (minutes). Because of the small number of cases the curves can be accepted only as suggestive of possible trends. It may be noted, however, that the group given 16 reinforcements during acquisition (I) responded more promptly after 17 minutes of spontaneous recovery than after $4\frac{1}{2}$ or 8 minutes, while the groups which were given only 8 reinforcements demonstrated a lesser or even reversed trend. It may be significant that at only the 17 minute interval does Group I show the effect of as much recovery as do the other 2 groups.

Groups IIa and IV

The comparison of Groups I, II, and III leads us to the conclusion that at least some of the differences between the 16-trial 16-reinforcement and the 50 percent reinforcement groups may be understood on the basis of the Group III (8/8) data. It may be said that, within the limits of the present experiment, the smaller number of reinforced trials results in a less-well established response as measured by reaction time, but in a more stable response as indicated by greater

resistance to extinction. This is true when the 8 reinforcements are given consecutively (8/8), and so might logically be expected to hold to some degree when the 8 reinforced trials are interspersed among non-reinforced trials (8/16). But the deviation during extinction of Group II (8/16) from Group I (16/16) is somewhat greater than the difference between Group III (8/8) and Group I, although this difference is not statistically reliable. What other factors are at work

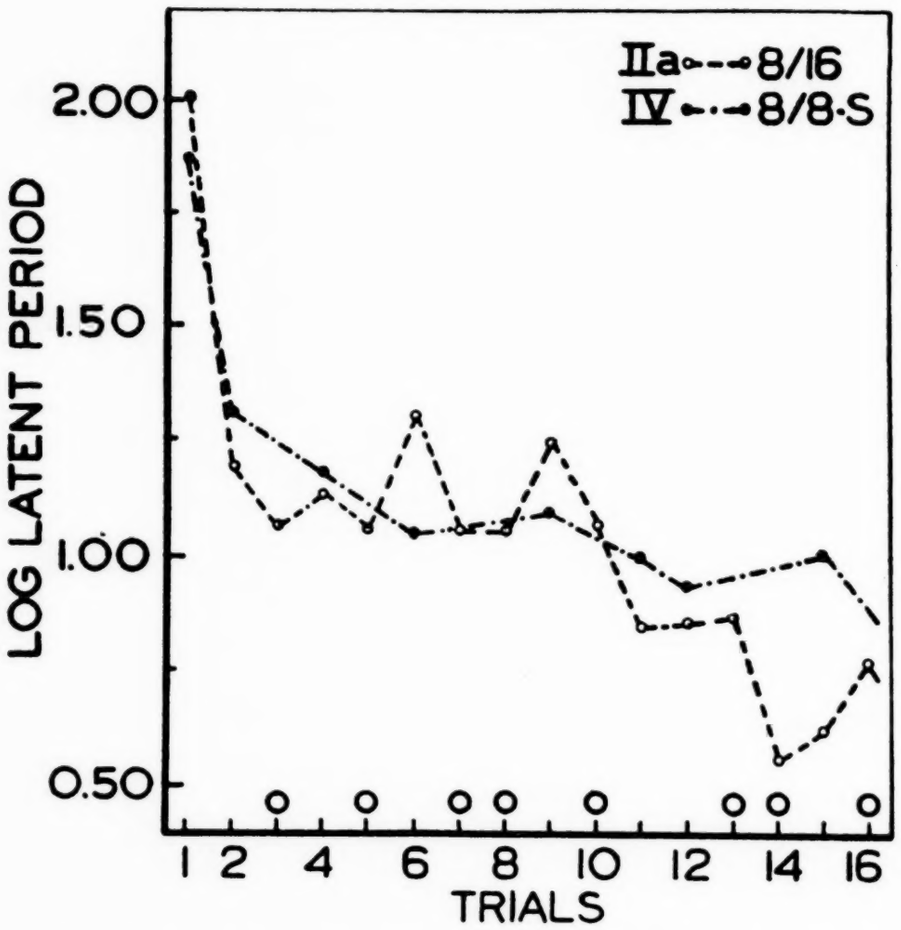


FIG. 3. Average latencies during acquisition series of Groups IIa and IV. The ciphers above the abscissa indicate those trials which were not reinforced in Group IIa. $N = 13$.

in Group II, leading to still greater resistance? It is immediately apparent that not only does the 50 percent reinforced group receive less reinforcement than does Group I, but that this reinforcement is spaced temporally. Many learning studies have demonstrated differences in retention resulting from differentially distributed practice. The question may therefore be raised whether or not the factor of temporal spacing of reinforcement contributes to the behavior of the 50 percent reinforced animals. For this reason Group IV was run, in which 8 100 percent reinforced trials were given, corresponding in time to the reinforced trials of Group II. But since an analysis of

variance indicated that litter difference contributes significantly to extinction scores, this group could not be compared directly to Groups I, II, and III, which were not matched in litter with Group IV. So Group IIa was added, identical in treatment with Group II and matched for age and litter with Group IV.

Acquisition.—Figure 3 shows the course of acquisition of the two groups. The 8 trials of Group IV have been spread on the graph to correspond temporally to the reinforced trials of Group IIa. For the first 10 trials of Group IIa the characteristic primarily differentiating the groups is the irregularity of the 50 percent reinforced animals: their latency increases after the non-reinforced trials (3, 5, 7–8). But in the last 6 trials of Group IIa the picture changes, and non-reinforcement (trials 10, 13–14, 16) is not followed by such consistent latency increases. As a result the last 6 trials of acquisition, as well as the first extinction trial, of Group IIa have shorter latencies than the corresponding points in the Group IV curve. The difference between the last 4 acquisition and the first extinction trials of IIa and the last acquisition and first extinction trials of IV is significant only at the $7\frac{1}{2}$ percent level.

Extinction.—Figure 4 shows the early course of extinction for Groups IIa and IV. No consistent difference is apparent between the 2 curves. Table 2 compares the number of trials required for

TABLE 2

AVERAGE NUMBER OF EXTINCTION TRIALS REQUIRED TO REACH THE CRITERION OF 3-MINUTE NON-RESPONSE

Group	Condition	*N*	Trials
IIa	8/16	13	22.5
IV	8/8–S	13	16.4
	Difference	*t*	*P*
IIa–IV	6.1	1.13	28%

extinction to the 3-minute criterion. Group IIa has an average of 22.5 extinction trials, while Group IV has a 16.4-trial average. The medians (20 and 13) confirm this trend, but the reliability of the difference is doubtful. The *t*-value is 1.13, which yields a *P*-value of 28 percent. Since the *P*-value of the extinctive difference between Groups II (8/16) and III (8/8) is larger than this (57 percent), the chances are greater that Groups II and III are statistically similar than that Groups IIa and IV are alike. The increase in average inter-trial interval in Group IV thus did not add to the extinction-resistance: at least as much resistance would have

resulted in these animals from the mere shifting from the condition of 16 reinforced trials to that of 8 reinforced trials.

Spontaneous Recovery.—As in the instance of the other groups, the points determining the spontaneous recovery curves of Groups IIa and IV represent too few cases to assume more than suggestive

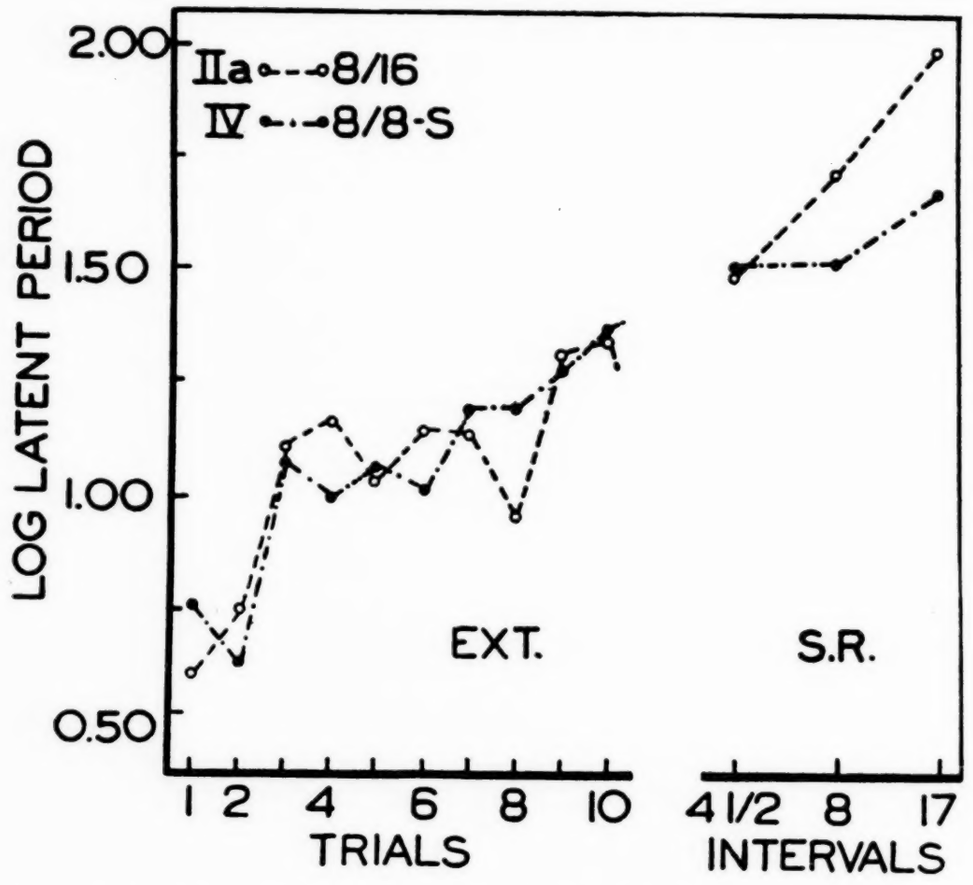


FIG. 4. Average latencies during the first 10 trials of extinction and during spontaneous recovery, for Groups IIa and IV. $N = 13$ for trials 1–3 of extinction, and is variable thereafter. $N = 3$–5 for each point of spontaneous recovery. See text.

significance. The trend seems to be opposite from the usual spontaneous recovery effect, that which was illustrated by the 16/16 group. The longer time elapsing since the end of the extinction series is associated with a slower, rather than a quicker response.

DISCUSSION

The data indicate that the condition of 16 reinforced acquisition trials leads to the most rapid response at the end of the acquisition series. The conditions of 50 percent reinforcement (8/16) and of 8-trials 8-reinforcements are characterized by significantly slower final response time, and are approximately equal to each other with respect to this measure. On the other hand, the condition of 16

reinforced trials is followed by the most rapid extinction. Certainly the 8/16 animals, and probably the 8/8 group, require more extinction trials than the 16/16 to reach the criterion of 'complete' extinction.

It is apparent that speed of response at the end of acquisition and resistance to extinction are not always parallel measures of the extent of learning. In fact, in the present study this relationship is more nearly inverse than direct, *i.e.*, the animals responding most promptly at the end of the acquisition series extinguish most readily. It may further be stated that the problem of reinforcement and non-reinforcement cannot be understood in terms of simple increments and decrements of learning strength or excitation level or reflex reserve. It cannot be said that every reinforced trial adds to the strength of the S-R connection and that every non-reinforcement is followed by a weakening of the connection. A somewhat more involved explanation must be forthcoming.

Perhaps the most striking finding of the present research is the direction of the extinction difference between the 16/16 and 8/8 groups. The administration of trials 9–16 of the Group I (16/16) acquisition series actually decreased the subsequent resistance to extinction. One might invoke an 'expectancy' hypothesis to explain the behavior of the 50 percent reinforcement group,[5] but the applicability of this hypothesis to the results of the 8/8 group is somewhat questionable. Indeed, it is unlikely that this extension would be made by those who postulate 'expectancy' to explain the effects of 50 percent reinforcement. Humphreys, for example, explicitly states that the increased resistance to extinction is "apparently due to [the subject's] difficulty in forming an hypothesis of continuous non-reinforcement after *discontinuous* reinforcement in acquisition" (**6**, p. 74, italics the present writer's). The relevant characteristic of the acquisition series as conceived in his theory is the *random* nature of the reinforcement, and not the relatively small *amount* of reinforcement.[6] Some further mechanism must be looked for to explain why 8 consecutive reinforced trials lead to the establishment of a longer-lasting response pattern than does the condition of 16 reinforced trials.

[5] The question may here be raised if an hypothesis of 'expectancy' can be the ultimate explanation for the behavior of a rat. It is possible that the development of 'expectancies' (or patterns of set or tension) may eventually be referred to certain underlying processes, such as excitation and inhibition, or in turn be 'explained' by a more quantitative hypothesis of neural functioning.

[6] In Humphreys' experiment on the human eyelid response, one group of subjects corresponded to our Group IV (8/8-S), but none to Group III (8/8). The magnitude of eyelid response for 24 extinction trials is greater for this group (48 reinforced and spaced trials) than for the 96-trial 100 percent reinforced group (significant at about the 5 percent level). This superiority is attributed by Humphreys to the factor of efficiency of spaced learning.

If it be thus assumed that some other mechanism is at work differentiating the 8-trial 8-reinforcement group from the 16-trial 16-reinforcement group, it might be argued that this unknown factor could apply also to the 16-trial 8-reinforcement group. (The slight additional resistance to extinction of Group II might be attributed to some positive factor involved in the non-reinforced trials of the 50 percent group, *e.g.*, tension reduction by the running, the 'excercise' of the response, or sub-goal reinforcement by the empty food box.[7]) For this reason, before 'expectancy' is invoked to account for the extinction scores of the 50 percent reinforcement rats in this experiment, the behavior during extinction of the 8-trial 8-reinforcement animals must be explained more satisfactorily.

Several possibilities present themselves in this regard. It might be suggested that the simple locomotor habit is complicated by a drive to explore the apparatus. In the course of their 16-trial acquisition series, the 16/16 animals would have had this exploratory drive relatively satisfied, and thus it would not carry over to the 'extinction' series. On the other hand, the animals of the 8/8 group would have had less opportunity to explore the apparatus in the course of their acquisition series. During the extinction trials, then, they would be impelled to leave the starting box by 2 forces: the learned S-R connection and the continuing urge to explore. The objection might be raised that such an explanation would lead one to expect no longer extinction for 8/16 than for 16/16, since their total numbers of acquisition trials were the same. To meet this objection it might be replied that the uncertainty of reinforcement in the 8/16 condition distracted the animals from exploring the apparatus during the acquisition series. The 16/16 animals, having 'solved' the reinforcement problem, would be more free to satisfy their urge to explore during the later acquisition trials. This explanation could be tested by measuring not only the latency of leaving the starting box but also timing the animal's entrance to the food box. (This additional measure has been used by Verplanck (**8**) in a discrimination study in this situation.) Time spent in exploring would be reflected particularly in this latter measure.

The very rapid initial extinction rate of Group I (16/16) may be understood in terms of Anderson's concept of externalization of drive (**1**). According to this theory the external conditions associated with the satisfaction of the hunger drive become themselves capable of arousing the drive mechanism. As more training is given in the

[7] Hull, in unpublished seminar notes of April 17, 1941, has suggested that the greater resistance of Humphreys' 50 percent reinforced group may be understood in terms of the conditioning of the response to the receptor discharge from the non-reinforced trials. The inter-trial intervals in the present study are probably too long to allow this process to operate to an appreciable extent.

constant situation the importance of these external factors for arousing the drive becomes relatively greater, and internal and external factors combine to bring the drive to a maximum. After much learning has taken place (the drive being greatly externalized), removal of the food-reward (change in the external factors) should result in a greater disturbance of the response than would removal of the reward earlier in the learning process, before the drive had become partially dependent upon the external conditions. This prediction is confirmed in the present experiment by the more rapid latency increase during early extinction for the animals having received 16 reinforcements (Group I) than for those having received 8 reinforcements (Groups II, III, and IV). Since the latency at the end of acquisition was much the lowest in Group I, this greater increase during extinction need not result in *slower* response for these animals; as has already been noted, the Group I latency increased 0.59 log units and Group II (8/16) only 0.19 log units in the first 4 extinction trials, but Group I was still responding the faster on the fourth trial. Up to this point our results are consistent with the theoretical formulation. At the end of acquisition the Group I (16/16) animals are responding the fastest because of the summed effect of the original internally-aroused drive plus the externalized drive. When the latter factor is removed or reduced (by omission of reinforcement) the total drive is suddenly decreased; this loss is greatest for Group I, in which the most externalization had originally taken place, and is reflected in the very rapid initial latency increase. But there is no reason to assume that externalization decreases the *absolute* potency of the internal factors for arousing the drive. After the externalized drive of Group I (16/16) has ceased to operate, the original internal factors still remain. Hence, following the initial disturbance of Group I caused by altering the external conditions, the groups should respond similarly, for all animals would be alike with respect to these internal factors (physiological conditions) which arouse the food-seeking drive. Therefore all groups should be approximately equivalent during the later stages of the extinction. This is not found to be true in the present experiment, since the 16-reinforcement group actually extinguished more readily than did the 8-reinforcement animals. The concept of externalization of drive thus does not account for the greater resistance to 'complete' extinction of the animals receiving the fewer acquisition trials.[8]

[8] It may be noted that Anderson's many predictions seem primarily to reflect the effect of varying conditions of motivation upon the manifestations of learning. No assumption is made as to the nature or the course of the underlying learning process, upon which the externalization of drive is superimposed. Nor is it clear whether the course of externalization is in itself to be regarded as a typical case of learning. Anderson believes that "this process is closely related to conditioning" (p. 206) but implies that the direct determination of its rate of progress would

A third possibility might assume that the frustration produced by withdrawal of reinforcement would be greater for those animals that had learned the running habit more strongly (Group I) than those that had had less reinforced practice (Groups II and III). This emotional condition, extending over a period of $\frac{1}{2}$ hour or more, would depress the level of excitation and lead most rapidly in the 16/16 group to the 3-minute non-response criterion. If the extinction series had been carried beyond this point to a different criterion, it is possible that the 16/16 rats would have recovered from the emotional blocking, resumed more prompt response, and perhaps outlasted the other animals.[9]

A fourth alternative suggests that reinforcement involves not only a positive effect (*e.g.*, increased excitation), but also a negative effect (*e.g.*, increased inhibition). That some sort of depressing influence is involved during repeated reinforcement has been reported by many investigators (*e.g.*, Pavlov (**7**, p. 248)), and has been posited as accounting for the fact of diminishing returns in acquisition, particularly with massed practice. One representation of this tendency is Hovland's 'inhibition of reinforcement' (**4**). The process as here conceptualized is of a somewhat different nature, depressing not so apparently final acquisition level as response during extinction trials. The size of the inhibitory increment produced by any extinction trial might conceivably be proportional to the amount of inhibition already present. This would be greatest in the 16-reinforcement group, and therefore inhibition would be built up most rapidly in these animals, leading to early extinction. The facts of spontaneous recovery suggest that this inhibition is dissipated with the passage of time. From this it would be predicted that if a 24-hour interval were interpolated between acquisition and extinction, the 16-reinforcement group would resist extinction longer than would the other groups, due to its higher level of excitation produced by the greater number of reinforcements during acquisition.

An additional item concerning the nature of this inhibitory process and its dissipation may be adduced from the results of Group IV (8/8-S). The time relations which were employed in this experiment were not such as to produce more efficient learning in Group IV, for Group III (8/8) approximates Group II (8/16) more closely than does Group IV. Spacing in this case gave no additional advantage that could be detected by the measures used. Apparently the in-

be most difficult. It may be that the unequivocal verification of such a conceptualization, positing as it does the interplay of two learned functions rather than the variation of one, will prove to be impossible.

[9] This represents essentially a suggestion offered by Dr. F. S. Keller, in a personal communication.

hibition is not appreciably dissipated in the space of time used as inter-trial intervals in Group IV. That an increase in inter-trial interval does not always produce more efficient learning of this type of response has already been demonstrated by Gagné (**2**).

This postulation does not of course imply that resistance to extinction is invariably in inverse proportion to amount of reinforcement during acquisition. Response level at any moment is the resultant of (at least) 2 opposing factors. If the positive factor has not been built up sufficiently by a minimum number of reinforcements, the response will not occur promptly, even though little of the negative factor is present. If fewer and fewer acquisition trials are given, the point will doubtless be reached beyond which the final level of the positive process will decrease more rapidly than that of the negative process, and less resistance to extinction will be encountered than when 8 or 16 reinforced trials are given.

There is one interesting characteristic of the acquisition curve that has quite consistently appeared in the several studies of this learning situation that have employed uniform reinforcement: the responses on the 5th, 6th, and 7th acquisition trials are less prompt than the response on the 4th trial (see, *e.g.*, **2**, **3**, **8**).[10] This temporary negative trend in the learning curve may represent the first accumulation of the inhibition produced by reinforcement. Decreasing the number of acquisition trials beyond this point should certainly lead to progressively more rapid extinction.

These suggested explanations for the resistance to extinction of Group III are of course only tentative, and intended primarily to stimulate further research. The only necessary conclusion is that experimental clarification of the relationship between extinction scores in Groups I (16/16) and III (8/8) must precede satisfactory explanation of the results of Group II (8/16). It would be premature at this juncture to invoke the principle of 'expectancy' to account for the effect of random reinforcement upon the response here investigated.

Summary

Five groups of 13 rats each were trained in a simple running response, with reinforcement furnished by food. The latency of acquisition, extinction, and spontaneous recovery trials was obtained, as well as the number of extinction trials required to reach a criterion of 3-minute non-response. Four conditions of reinforcement were used:

[10] The Group III (8/8) curve in the present experiment is the only one of those examined that does not demonstrate this trend. There is no obvious reason for this lack of agreement. However, if the latencies of Groups I and III are combined for trials 4, 5, 6, and 7, this apparent deviation is greatly reduced. The respective log values are then 0.855, 0.86, 0.86, and 0.81.

Group I—16 acquisition trials, 16 reinforcements.
Groups II and IIa—16 trials, 8 reinforcements (in random order).
Group III—8 trials, 8 reinforcements.
Group IV—8 trials, 8 reinforcements, spaced to correspond temporally to the reinforcements of Groups II and IIa.

1. The animals receiving 16 reinforcements (Group I) had a shorter latency at the end of acquisition than did those receiving 8 reinforcements (Groups II, III, IV).

2. The extinction curve of the group given 16 reinforced acquisition trials (I) exhibited the most rapid initial rise.

3. The number of trials required for extinction was less for the group having had 16 reinforcements (I) than for the groups having had only 8 reinforcements during acquisition (II, III, IV).

4. No clearly significant difference was observed among the groups given 8 reinforcements in various combinations.

The theoretical implications of the data are discussed. It is suggested that some more comprehensive and quantitative hypothesis than 'expectancy' be evolved to account for the results. Several possible lines of experimental approach are outlined.

(Manuscript received May 27, 1941)

References

1. Anderson, E. E., The externalization of drive. I. Theoretical considerations, *Psychol. Rev.*, 1941, **48**, 204–224.
2. Gagné, R. M., The effect of spacing of trials on the acquisition and extinction of a conditioned operant response, *J. exp. Psychol.* (in press).
3. Graham, C. H., and Gagné, R. M., The acquisition, extinction, and spontaneous recovery of a conditioned operant response, *J. exp. Psychol.*, 1940, **26**, 251–280.
4. Hovland, C. I., 'Inhibition of reinforcement' and phenomena of experimental extinction, *Proc. Nat. Acad. Sci.*, 1936, **22**, 430–433.
5. Humphreys, L. G., The effect of random alternation of reinforcement on the acquisition and extinction of conditioned eyelid reactions, *J. exp. Psychol.*, 1939, **25**, 141–158.
6. Humphreys, L. G., Extinction of conditioned psycho-galvanic responses following two conditions of reinforcement, *J. exp. Psychol.*, 1940, **27**, 71–75.
7. Pavlov, I. P., *Conditioned reflexes* (Trans. by G. V. Anrep), London: Oxford Univ. Press, 1927, pp. xv + 430.
8. Verplanck, W. S., The development of discrimination in a simple locomotor habit (to appear).

THE MAGNITUDE OF SIMULTANEOUS COLOR CONTRAST AND SIMULTANEOUS BRIGHTNESS CONTRAST FOR CHIMPANZEE AND MAN [1]

BY WALTER F. GRETHER

Yale Laboratories of Primate Biology

Although simultaneous contrast has received considerable study with human subjects, there is very little information about the part it plays in animal vision. For the rat simultaneous brightness contrast has been demonstrated by Lashley (**11**). When three equally gray patches were presented on light, medium, and dark backgrounds, a rat previously trained to choose the lightest of three grays showed a preference for the gray on the dark background. Another rat accustomed to choosing the darkest of three grays preferred the gray with the light background. Révész (**13**) has demonstrated simultaneous color contrast in chickens. After training them to peck at rice grains only on green surfaces, he found that they would also peck at rice grains on gray areas bordered by red. By a similar method Kühn (**10**) demonstrated color contrast in the bee. Studies by Ewald (**2**) on *Daphnia*, and Knoll (**9**) on hawk-moths claim to have shown the effect of simultaneous brightness contrast on phototropic responses. The results could be explained more simply, however, as response to brightness gradients without true contrast necessarily being involved. In none of the previous studies of simultaneous contrast in animals have any attempts been made to determine the magnitude of the demonstrated effects.

Helmholtz (**5**), in reconciling contrast phenomena with his theory of color vision, proposed that these effects could be explained as illusions or errors of judgment. Since judgment must occur at a relatively high level of integration in the central nervous system, Helmholtz saw no necessity for explaining contrast by his theory of retinal function. Some of the facts about simultaneous contrast would seem to be in essential agreement with such a view. The magnitude of the effect, for example, is greatly influenced by the degree to which the objective nature of the stimulating situation is concealed. A gray patch on a colored background gives little contrast if there is any visible break between the two surfaces. Concealing this break by a covering of tissue paper greatly enhances the

[1] The term color contrast is used in this paper only in the more restricted sense of chromatic, as opposed to achromatic, contrast. For the achromatic effects the term brightness contrast is used.

contrast effect, in spite of a marked reduction in saturation of the background which this involves. Conversely, if two gray areas on unlike backgrounds are joined by a strip of the same gray, the contrast effect vanishes.

One could logically expect, from Helmholtz's explanation and the above-mentioned dependence of contrast upon concealment of the objective nature of the situation, that contrast would be considerably affected by the subject's knowledge of what is being tested and the resulting attitude or set toward the test stimuli. In human beings

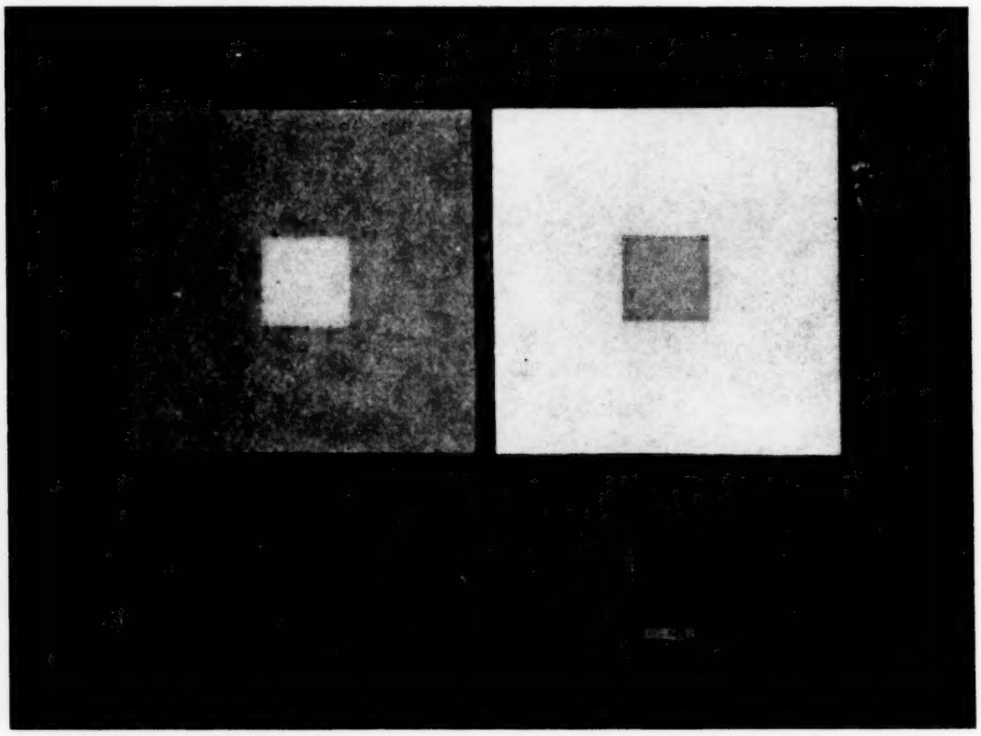


FIG. 1. Front of contrast apparatus with light and dark backgrounds in position for brightness contrast.

(**8**), however, contrast is exceedingly resistant to the attitudes of the subject. It differs in this respect from color and brightness constancy which vary considerably with the subject's attitude toward the stimuli (Henneman, **6**). This effect of attitude was reflected in Locke's (**12**) finding that brightness constancy is greater for the rhesus monkey than for man. In the present comparison of the magnitude of simultaneous contrast in chimpanzee and man we have an extreme difference in attitude on the part of the subjects. The chimpanzees obviously could have had no possible anticipation of what was to occur when contrast stimuli were introduced. The human subjects, on the other hand, were fully aware of the nature and purpose of the test.

Apparatus.—In measuring simultaneous contrast with chimpanzee subjects the primary problem was the construction of a suitable apparatus. Of first importance was some arrangement whereby the animals could be trained to respond selectively to the desired stimuli, to the exclusion of the surrounding areas by which the contrast effects were induced. It was necessary, also, to have the stimuli and their surrounding areas or backgrounds independently adjustable. These conditions had to be met, furthermore, without visible break between stimuli and backgrounds if maximum contrast were to be obtained.

The apparatus,[2] designed and constructed to meet the above requirements, is shown in Figs. 1 and 2. In the photograph of Fig. 1 is shown the front of the apparatus set up for bright-

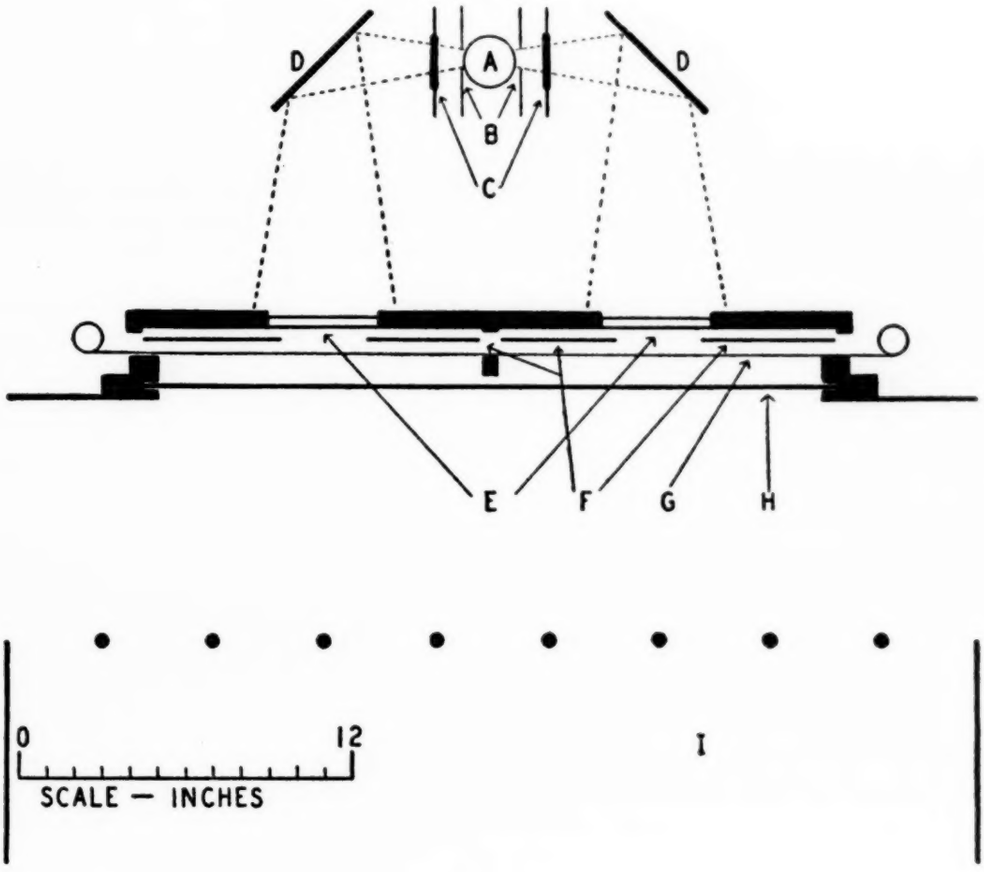


FIG. 2. Horizontal cross-section of contrast apparatus. *A*, source light; *B*, diaphragms; *C*, Wratten color filters; *D*, mirrors; *E*, plate glass windows; *F*, cardboard backgrounds; *G*, tracing paper covering (*E*, *F*, and *G* actually in contact, but for clarity of illustration are separated in figure); *H*, sliding panel; *I*, experimental cage.

ness contrast. Further details are shown in the horizontal cross-section of Fig. 2. The front of the apparatus consisted of a vertical plywood panel, 56 inches high by 35 inches wide, and painted a flat, medium gray. At the center of this panel was a rectangular opening, 24 by 48 inches, in which the stimulus fields were presented. A vertically sliding panel (*H* in Fig. 2) could be lowered to cover the stimuli.

The stimuli to which the chimpanzees were trained to respond were 3-inch squares presented in the center of each half of the rectangular stimulus field. These 3-inch squares were formed by openings cut into sheets of cardboard tha. formed the surrounding area. Behind these cardboard sheets, overlapping the 3-inch square openings, were two plate-glass windows, hinged at the top. The bottom of each window rested against a plunger, in which there was a narrow notch supporting a food shelf. Thus, slight pressure against the window (with 1/16 inch of move-

[2] Helpful suggestions regarding the design of this apparatus were received from Doctor Vincent Nowlis.

ment) moved the plunger and released the food shelf. A chute carried the food into a pan below the corresponding stimulus. Over the surface of the cardboard and the windows forming the stimulus squares was stretched a covering of tracing paper through which the color of the cardboard was clearly visible. By the use of a continuous roll it was possible to move a clean surface into place whenever the tracing paper became soiled. A window shade roller was used to hold the clean paper. The spring in the roller served to maintain a continuous tension and prevent wrinkling of the paper.

To produce the necessary changes in background color the cardboard sheets surrounding the stimulus areas could be removed and any other desired color substituted. The colors of the stimulus squares were provided by light transmitted from behind, as shown in Fig. 2. Brightness and color were controlled by diaphragms and Wratten color filters. Since the apparatus was used with the front illuminated by a ceiling light in the room, the actual appearance of the stimulus squares was determined by the combination of reflected and transmitted light. The backgrounds received only this reflected light, which was, of course, the same at all times.

As can be seen in Fig. 2, the barred front of the experimental cage was separated some distance (16 inches) from the contrast apparatus. This insured having the animals' eyes far enough from the stimuli that both would be visible at the same time. Having to reach this distance also effectively discouraged the animals from responding to both stimuli at the same time. The experimenter was at all times hidden from the animals by a screen. Observation of the animals was made through a small opening (2½ inches in diameter) in the screen.

Subjects.—The animals used in this investigation were three immature, laboratory-born chimpanzees, all of them males. At the time of the experiment two of these, Tom and Dick, were approaching age seven years; the other animal, Hal, was about four and one-half years old. Because of their previous use in tests with spectral colors (Grether, 3), these three animals were very proficient in making color discriminations. One of the animals, Hal, was used only in the first experiment on color contrast. Also, for purposes of comparison, six adult human subjects (one woman and five men) were tested.

Experiment No. 1. Simultaneous Color Contrast

Test Conditions and Preparatory Training.—To produce the simultaneous color contrast, sheets of red and green (shamrock green) cardboard were used to provide the backgrounds. The particular colors chosen had almost identical reflectance under incandescent illumination, as was necessary in order to insure color without brightness contrast. Both the red and green were high in saturation and were quite pure in the sense that they contained a minimum of blue or yellow. The attempt to find a more nearly complementary color combination (such as red and blue-green) of equal brightness and high saturation proved unsuccessful.

Color closely matching the backgrounds in hue was projected into the stimulus squares from behind (see Fig. 2) by using Wratten filters 29F (red) and 58A (green). With incandescent light the green filter transmitted about 20 percent higher brightness than the red. The brightness of this filtered light was controlled by diaphragms between the light source (a 60-watt frosted lamp) and the filters. A graded series of diaphragms was used, each a separate metal plate with a hold drilled into it. The size of these diaphragms was arranged so that each gave a brightness about half that of the next larger one in the series. Even with the smallest holes possible with the available drills, the color transmitted through the stimulus windows and tracing paper was too bright for threshold work. To reduce this transmission the back surface of the stimulus windows was covered with a thin, light gray paper. Using gray instead of white paper made the reflectance of the stimulus windows more nearly equal to that of the gray and colored backgrounds. Table 1 gives the brightness values, measured at the front of the stimulus windows, obtained with the series of diaphragm apertures. Measurements were made separately with a Macbeth illuminometer and a Weston phototronic cell equipped with a Viscor filter. This cell and filter combination has a sensitivity curve very nearly equated to the normal human photopic visibility curve. Measurements were made with this cell connected to a sensitive Leeds & Northrup galvanometer. The brightness measurements obtained by these two methods were found to agree very closely, and were pooled to give the values in Table 1. In measuring the brightness of the white light the Wratten color filters were replaced by a dummy filter of the same size made of clear glass.

In a study of simultaneous contrast considerable attention must be paid to the brightness relationships in the visual field. These were measured in the present study with a Macbeth

illuminometer and were found to be as follows: The gray panel forming the front of the apparatus varied in brightness from 1.0 millilambert near the bottom to 1.7 millilamberts at the top. The 3-inch square stimulus patches, with no transmitted light added, were 2.75 millilamberts. The brightnesses of the backgrounds were: gray, 1.85 millilamberts; red, 1.98 millilamberts; green, 2.06 millilamberts. These measurements of the stimulus patches and backgrounds were made with the tracing paper covering in position.

Training of the chimpanzees in preparation for the contrast test was begun with equally gray backgrounds and with red and green transmitted through the stimulus patches. Since the animals had been accustomed in preceding experiments to take the longer wave-length of two spectral colors, red was used as the positive stimulus in the present study. Beginning with large diaphragm apertures and bright colors, the apertures were gradually reduced to train the animals for near-threshold discrimination. Since the white light reflected from the front of the stimulus patches was constant (2.75 millilamberts), reducing the amount of transmitted color had the effect of reducing the ratio of color to white, and hence of reducing the saturation of the

TABLE 1

THE BRIGHTNESS OF TRANSMITTED WHITE, RED, AND GREEN LIGHTS AT VARIOUS APERTURE SIZES IN CONTRAST APPARATUS

Aperture Number	Aperture Diameter	Brightness of		
		White	Red	Green
	in.	*ml.*	*ml.*	*ml.*
1	3/64−	.00824	.00124	.00148
2	1/16	.0151	.00227	.00272
3	5/64+	.0284	.00426	.00511
4	1/8	.0536	.00804	.00965
5	11/64+	.0913	.0137	.0164
6	1/4	.183	.0274	.0329
7	3/8	.364	.0546	.0655
8	1/2	.660	.0990	.119
9	3/4−	1.13	.169	.203
10	Full opening	1.75	.263	.315

stimuli. The color transmitted by the red and green Wratten filters was sufficiently similar in brightness to permit use of the same aperture size for both filters.

As a means of developing a high degree of proficiency on the part of the animals in making discriminations between red and green when quite unsaturated, an actual threshold determination was carried through. After the approximate limits of each animal's discrimination had been found, a series of five apertures was chosen, such that the largest gave colors easily discriminated, and the smallest gave colors below the 75 percent discrimination threshold. During each 25-trial experimental session, 5 successive trials were given at each aperture size, beginning with the largest and ending with the smallest of the series. This series was then repeated in successive sessions (a total of 8) until 40 trials had been completed at each aperture size. At the conclusion of this preparatory training the animals were ready for the actual contrast tests, for which the method is described in the following section.

Results of Test for Simultaneous Color Constrast.—In the training procedure preceding the tests for contrast, the animals' thresholds were determined for discrimination between red and green as the saturation of these two colors was reduced. The discrimination data for these threshold determinations, and the thresholds computed by linear interpolation for the 75 percent point, are given in Table 2. These values, although of no direct significance for the present study,

give a measure of the chimpanzees' capacity for discrimination of slightly saturated colors. In an earlier determination (Grether, 4) of the spectral saturation curve for chimpanzee, similar thresholds had been measured, with which those given here agree closely in magnitude.

TABLE 2

RESULTS OF THRESHOLD DETERMINATIONS PREPARATORY TO SIMULTANEOUS COLOR CONTRAST TEST

Aperture Number	Saturation Ratio Color/White	Percent Correct Choices in 40 Trials		
		Tom	Hal	Dick
6	.0109	100.0	97.5	97.5
5	.00546	100.0	95.0	97.5
4	.00321	85.0	80.0	85.0
3	.00170	62.5	72.5	80.0
2	.00100	67.5	67.5	57.5
Threshold saturation ratios		.00235	.00201	.00154

The actual tests for simultaneous contrast were made by substituting red and green for the gray backgrounds which had been used up to this point. The right-left positions of these were interchanged in an irregular manner. These red and green backgrounds had never before been seen in the apparatus by the chimpanzees. Now with all colored light eliminated from the central stimulus squares, and with both stimuli baited, the animals were given an opportunity for choice. Under these conditions the green-surrounded stimulus square appeared slightly reddish, the red-surrounded one slightly greenish, to human subjects. If the same were true for chimpanzees they should now, because of the trained preference for red, choose the green-surrounded square. This was, indeed, what the chimpanzees did on most of these critical trials. The results are summarized in Table 3.

TABLE 3

RESULTS OF SIMULTANEOUS COLOR CONTRAST TEST

Subjects	Percent Positive Choices in 25 Trials				
	Contrast Test			Contrast with Brightness Control	
	1st Session	2nd Session	3rd Session	1st Session	2nd Session
Tom	96	100		100	100
Hal	64	96	100	100	100
Dick	84	60	100	92	88

Of particular importance were the animals' responses on their very first trial of the contrast test, in which they encountered the colored backgrounds for the first time. As might be expected to result from such a sudden change in the test conditions, the animals hesitated and delayed unusually long before attempting a choice. The first animal, Tom, after some hesitation, pushed timidly against the red background. When this failed to bring a reward he touched the stimulus square within this red background. Because of the double-baiting procedure he was rewarded for this choice, even though it was in opposition to the contrast effect. On the second trial he again touched the red background lightly, but then shifted over to the green-surrounded square. On the remaining 23 trials of this session he promptly chose the green-surrounded stimulus. Thus, although he was rewarded for a negative choice on the first trial, all following choices in this session were positive (*i.e.*, indicating contrast).

For the second animal, Hal, the results gave somewhat less clear-cut evidence for simultaneous contrast. On the first trial Hal, like Tom, pushed against the red background, but so near the stimulus square that the food reward mechanism was released. Nevertheless, the green-surrounded square was chosen on the second trial. But in the 25 trials of this first contrast session only 16 choices were positive. Unfortunately for the experiment, Hal had entered one of his occasional lapses in motivation on the day this contrast test was begun. Although he completed the session, he obviously lacked his normal interest in experimentation. It is not improbable that the change in test conditions contributed to his low motivation. After an interval of a month, during which Hal had been given 125 trials on the original red-green discrimination with equal gray backgrounds, he was returned to the contrast test. With his motivation much improved, he now made only one negative choice in a total of 50 trials, clearly indicating the influence of the color contrast.

The third chimpanzee, Dick, likewise, gave positive results. On the first trial this animal, unlike the other two, responded first to the green, then to the red gackground, and finally to the green-surrounded stimulus square. Thus his first choice of a stimulus square was positive, as were also his responses on the next 5 trials. In the 25 trials of this first session, however, he made four negative choices, of the red-surrounded stimulus. During the next session, Dick apparently learned that choice of either stimulus brought a reward, and made approximately random choices (15 positive in 25 trials). This was undesirable for continued use of him in the experiment, and he was therefore retrained with the red and green backgrounds in place, but with sufficient red and green added to the stimulus squares to overcome the contrast effect. Now only the red stimulus was baited,

and at times this had a red, and at other times a green, background. After one session of this training Dick was returned to the contrast test, and made 25 positive choices in 25 trials.

Although the red and green backgrounds were almost identical in brightness to human beings, this might not have been true for the chimpanzees. It was recognized that the animals might, therefore, have been responding to brightness rather than color contrast. This was entertained as a possibility, even though none of these chimpanzees had ever been trained in brightness discrimination. For this reason a control test was made in which the brightnesses of the stimulus squares were rendered unequal by transmitted white light. This was added an equal number of times to the red- and green-surrounded stimulus squares. This brightness difference was sufficient to overcome any reasonable degree of brightness contrast, but did not appreciably disturb the animal's continued choice of the green-surrounded stimulus square. The results of this control are included in Table 3.

Magnitude of Simultaneous Color Contrast.—Although the preceding tests left little doubt as to the occurrence of simultaneous color contrast for the chimpanzee, the measurement of the magnitude of this effect makes the results considerably more meaningful. This was done by adding color to the central stimulus squares, and determining the saturation required in these for the contrast effect to become neutralized. To accomplish this red was transmitted through the red-surrounded, and green through the green-surrounded stimulus square, thus cancelling the color contrast effect. In this test also, double baiting was used, to avoid differential training to either background. During each session, the amount of this added color was changed every 5 trials, in an irregular manner. A total of 40 trials was given at each saturation value. The several degrees of saturation of the stimulus squares used during each session were such that on approximately 50 percent of the trials the contrast effect was overcome by the added color, as indicated by the reversal in the animals' choices. In spite of the double-baiting procedure throughout this determination, all three animals maintained their habit of choosing the more reddish appearing stimulus, regardless of background. The results of these determinations are shown as discrimination curves in Fig. 3. These curves show how, as the saturation of the stimulus squares was increased, the animals' choices changed from almost 100 percent choice of the green-surrounded stimulus to an almost complete reversal of this habit. Where these cross the 50 percent line, presumably, the two stimuli appeared alike to the animals. These crossing points were taken to indicate the amounts of

added color required to neutralize the contrast effect, and thus provide a basis for comparing its magnitude for chimpanzee and man.

The dash line in Fig. 3 gives the composite curve for the six human subjects tested on the same apparatus with the same stimulating conditions. These subjects were all told the object of the experiment, namely, to measure the magnitude of simultaneous color contrast. They were instructed merely to indicate which of the two central gray stimulus squares appeared to be the more reddish. The several saturation values were given in irregular order, and each presented a

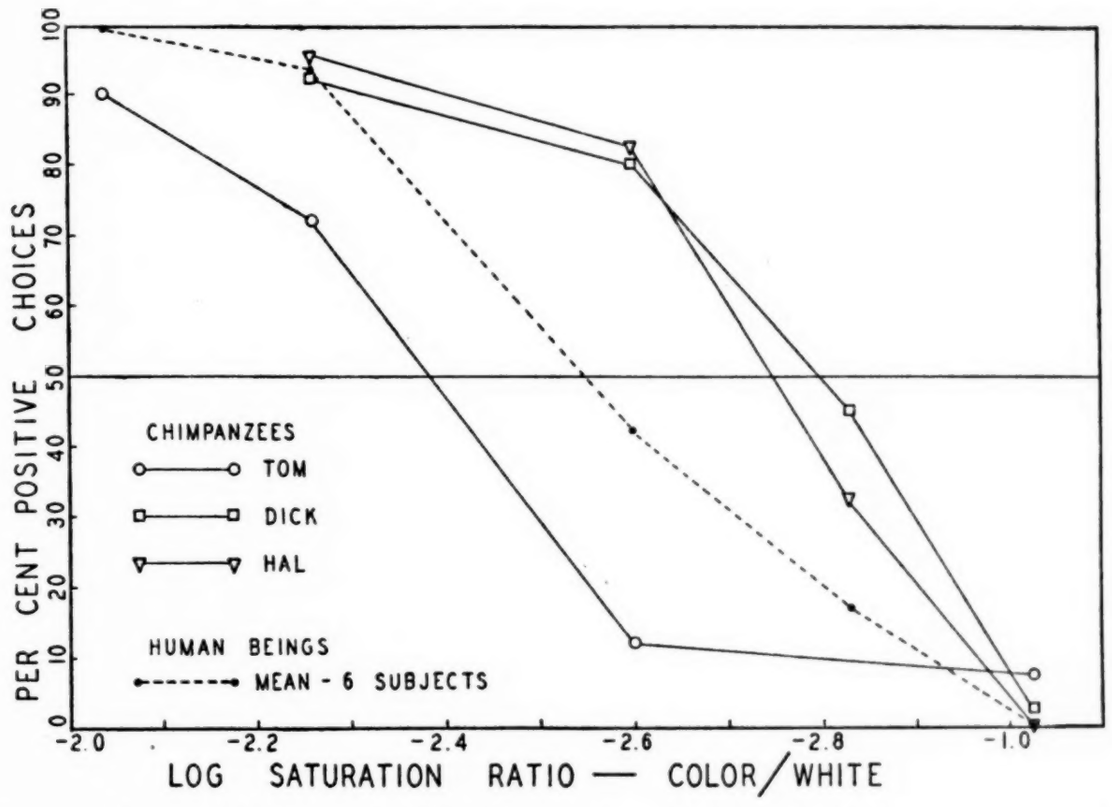


FIG. 3. Discrimination curves for determination of magnitude of simultaneous color contrast.

total of four times (not successively). At saturation values rendering the two stimuli very nearly equal in appearance the human subjects experienced difficulty in making the required judgment, and frequently reversed a previous decision. Higher and lower values were easily judged. Three of the six human subjects who were graduate students in psychology did not differ noticeably from the other three who were not psychologically trained. It is readily apparent from Fig. 3 that the magnitude of the contrast effect is highly similar for the chimpanzees and human beings. For Tom the contrast effect was slightly smaller, for Hal and Dick slightly greater, than for the human subjects.

Experiment No. 2. Simultaneous Brightness Contrast

Test Conditions and Preparatory Training.—In this experiment the brightness contrast was produced by using white and black cardboard sheets for the backgrounds. The black, however, was greatly attenuated by the tracing paper covering. Figure 1 shows the appearance of the stimulating fields with the black and white cards in the apparatus. During the preparatory training the same gray cards were used as in the preceding experiment. With the tracing paper covering in position the brightness of the white, gray, and black backgrounds was respectively: 3.74 millilamberts; 1.85 millilamberts; and 1.61 millilamberts. The stimulus squares as before without transmitted light, were 2.75 millilamberts.

As preparation for this experiment the two chimpanzees, Tom and Dick, were trained in brightness discrimination. While using the gray cards as backgrounds, the transmitted light was added to one of the stimulus squares, this being the positive or rewarded choice. As in the color contrast experiment, threshold determinations were carried out, at the conclusion of which the animals were highly proficient in discriminating very small brightness differences.

Results of Test for Simultaneous Brightness Contrast.—Of only incidental interest for the present study are the determinations of thresholds for brightness discrimination of the two chimpanzee subjects. Since such measurements had not been made previously for chimpanzee, these results will be published as a separate study. The actual thresholds, however, as computed by linear interpolation for the point of 75 percent discrimination success, were as follows: DI/I was .023 for Tom, .042 for Dick.

The first animal tested on this problem, Tom, was introduced directly to the contrast test from the threshold training with gray backgrounds. To the experimenter's surprise, Tom chose the white-surrounded, darker appearing stimulus square on the first 7 trials, in opposition to the contrast effect. Although he was rewarded for these choices, he made an abrupt reversal and chose the dark-surrounded, or lighter appearing stimulus 17 times in the remaining 18 choices. In the following session only the dark-surrounded stimulus was chosen. Thus, except for the first 7 trials, the results gave clear evidence of the operation of brightness contrast. A possible explanation for his choices on the first 7 trials is that he was responding according to background instead of stimulus brightness.

Instead of transferring the other animal, Dick, directly to the contrast test after the threshold training with gray cards, additional training was given in brightness discrimination with both backgrounds either white or black. Thus Dick, unlike Tom, had seen both the white and black cards in the apparatus (though not at the same time) before the actual contrast test. It was expected that this preparation might prevent his responding to the background brightness, as Tom was thought to have done on the initial trials. In the first session of the contrast test Dick was quite indiscriminate in his choices. In the 25 trials only 14 choices were of the dark-surrounded, lighter appearing stimulus. Moreover, the 11 choices of the darker appearing stimulus were distributed quite irregularly throughout the

session. In the second contrast session, however, Dick consistently chose the dark-surrounded stimulus, in accordance with the contrast effect.

Although the performance of Tom and Dick was somewhat ambiguous on their first experience with simultaneous brightness contrast, they both gave quite definite evidence of the effect in later trials. Nevertheless, the conclusion that chimpanzees experience brightness contrast is not as well supported by the data as the conclusion regarding color contrast.

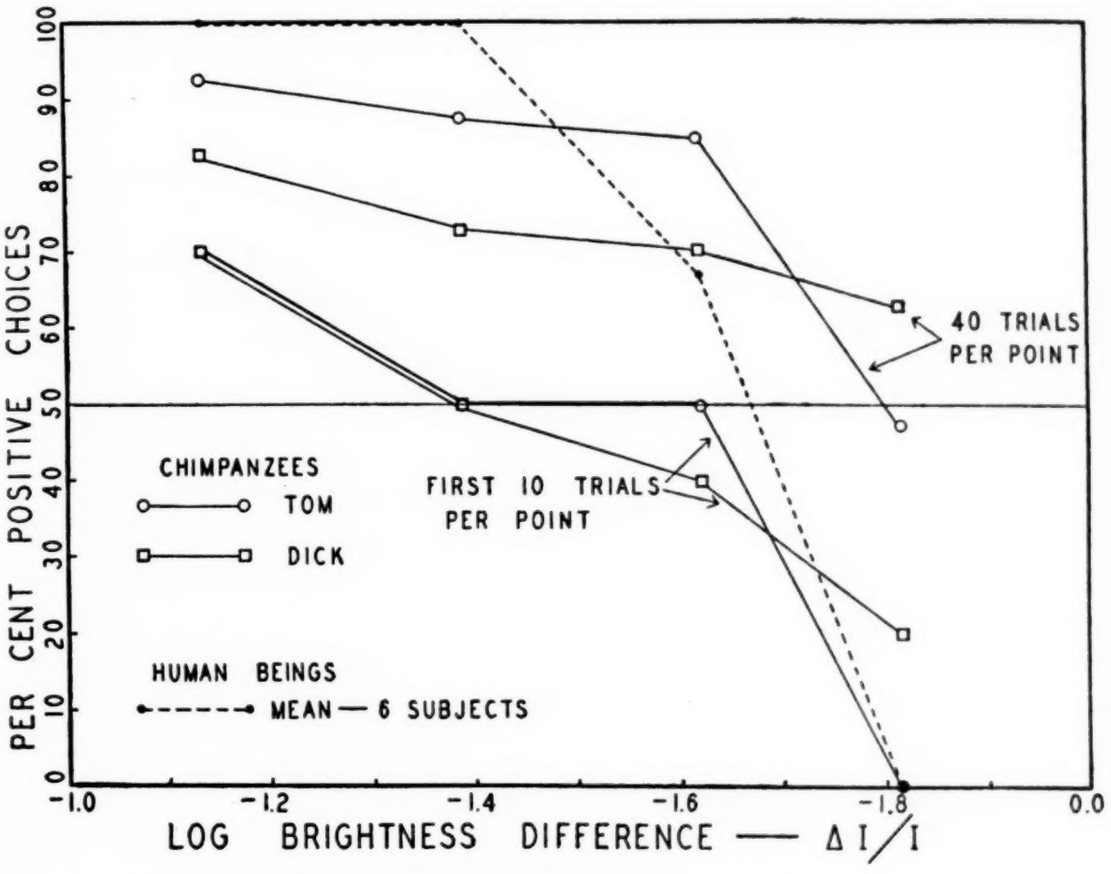


FIG. 4. Discrimination curves for determination of magnitude of simultaneous brightness contrast.

Magnitude of Simultaneous Brightness Contrast.—The measurement of the magnitude of brightness contrast was made in much the same manner as for color contrast in the first experiment. To neutralize the contrast, transmitted white light was added to the white-surrounded, darker appearing stimulus square. The intensity of this added light was changed every 5 trials in an irregular manner, until a total of 40 trials had been given at each added brightness. Figure 4 shows the results of this determination, with comparison data for the same group of human subjects used in experiment no. 1.

It will be noted in Fig. 4 that the curves for the chimpanzees are quite different when only the first 10 trials per point are used than when the total 40 trials at each point are considered. This difference resulted from a gradual deterioration of the animals' discrimination habits. By the time 40 trials had been completed at each test point both chimpanzees were responding consistently to the stimulus square with the black background, regardless of how much brightness was added to the other. Apparently the brightness discrimination habit had not been established firmly enough to persist throughout the magnitude measurements, in which choice of either stimulus was rewarded. The failure of a similar effect to occur in the color contrast test might well have been due to the far greater amount of training these subjects had been given in color discrimination in preceding experiments.

Because of the gradual breakdown of the animals' discrimination habits in the determination of magnitude, the curves in Fig. 4 for the first 10 trials at each point are, without doubt, the more meaningful for the comparison with human subjects. These chimpanzee curves fall somewhat toward the left (toward a smaller brightness difference) of the composite human curve. This would tend to indicate a somewhat smaller brightness contrast effect for the chimpanzees by comparison with man. The actual magnitude of the contrast effect can be computed in terms of brightness difference at which the curves in Fig. 4 cross the 50 percent line. This crossing point for the human curve is at a log value of -1.67, corresponding to a brightness difference of 47 percent (*i.e.*, $DI/I = .47$). The brightness difference at which Tom's curve drops below the chance line is 42 percent. For Dick this value is 25 percent. The discrepancy between chimpanzees and human beings on this test is insufficient to justify a conclusion of a difference in susceptibility to brightness contrast.

Discussion

First Response on Contrast Test.—Of special interest are the responses made by the chimpanzees on the first trials of the contrast tests. In experiment no. 1, it will be recalled, two of the three animals responded to the red background as their first choice. This is quite understandable if we consider that the animals had been accustomed to push against the most red area in the stimulus field. In the color contrast situation the red background was clearly the most red area. We could say, in Lewin's terms, that the red background had the highest positive valence in the particular field. On the later trials the chimpanzees made the next best choice, namely the reddish appearing square on the green background. The same type of first

response did not appear in the brightness contrast experiment. By this stage of the investigation the chimpanzees probably were better trained to push only the central stimulus squares.

The Specificity of the Response-determining Stimulus Area.—Before the present investigation was undertaken it was expected that the chimpanzees might fail to respond selectively to the central stimulus squares, but allow the background as well to determine their choices. Thus, when the red and green backgrounds were introduced, the animals might have chosen the red-surrounded square, because of the preponderance of red in this half of the stimulus field. Had it occurred, such a lack of specificity of the response-determining area would have prevented the demonstration of simultaneous contrast. The outcome of the investigation indicated that in most cases, at least, the animals were responding on the basis of color in the central stimulus squares rather than the backgrounds.

Significance of Results for Theories of Contrast.—The results of this investigation showed that chimpanzees, with no possible knowledge of the phenomenon being tested, were affected by simultaneous color and brightness contrast little if any different in magnitude from that experienced by human subjects. Such a result indicates, certainly, that contrast is relatively unaffected by our anticipations of what we are to see, and by our knowledge of what parts of the stimulating field are crucial for inducing the effect. The results show, furthermore, that the more or less formal color education which all of us receive is not a factor in determining the magnitude of simultaneous contrast. From this we can by no means conclude that contrast is independent of learning, although such may well be the case. Without doubt chimpanzees learn to make many discriminations on the basis of color and brightness during the course of their normal lives. But the more complex color training which human beings receive appears to have no effect on contrast.

It has been concluded by a number of investigators (Hering, **7**; Sherrington, **14**; Brückner, **1**; see also review by Tschermak, **15**) that simultaneous contrast is mediated at a relatively low level in the nervous system, probably the lateral geniculate body or retina. Although the results of the present study would not necessarily indicate subcortical functioning, they would agree in locating the contrast mechanism at a neural level in which higher associative and integrative functions of the cortex play no significant rôle.

Unfortunately Helmholtz's explanation of contrast as an illusion of judgment is too vague to be easily disproved. But since judgment is ordinarily considered to be a complex type of mental activity we could justifiably expect that it would be influenced by the knowledge and expectations of the subjects. This clearly was not found to be

true for the comparison of simultaneous contrast in chimpanzees and human beings.

Locke (**12**) has shown that brightness constancy is greater for rhesus monkeys than for man. The fact that no comparable difference was found between chimpanzees and man in the magnitude of simultaneous contrast adds support to the belief that contrast is a more stable and basic sensory process than constancy, and less influenced by subjective processes.

Summary and Conclusions

By means of a specially designed apparatus and a suitable training technique chimpanzees were tested for simultaneous color contrast and simultaneous brightness contrast. The chimpanzees were trained to indicate a choice by pushing against one of two 3-inch square stimulus areas, each within a separate background. The background and stimulus area could be changed independently in color, brightness, and saturation. After suitable preparatory training on discrimination of fine color or brightness differences in the stimulus areas, these areas were made identical, but given contrast-producing backgrounds (red and green for color contrast, black and white for brightness contrast). The choices of the animals clearly showed the influence of simultaneous contrast.

In addition to demonstrating the existence of simultaneous contrast for chimpanzees, the magnitudes of these effects were measured and compared with similar data for human subjects. These measurements of magnitude were made by finding the physical differences required in the stimulus areas in order to make them appear equal. The following conclusions may be drawn from the data:

1. Simultaneous color contrast and simultaneous brightness contrast operate in chimpanzee vision.

2. The simultaneous contrast effects are of approximately the same magnitude in chimpanzee and man.

3. The high degree of similarity in the effects for chimpanzee and man lends support to the conclusions of earlier investigators that contrast is mediated at a relatively low level in the optic system.

(Manuscript received May 25, 1941)

References

1. Brückner, A., Über Anpassung des Sehorgans, *Schweiz. med. Wschr.*, 1925, **6**, 245–252.
2. Ewald, W. F., Versuche zur Analyse der Licht- und Farbenreaktionen eines Wirbellosen (Daphnia pulex), *Z. Sinnesphysiol.*, 1914, **48**, 285–324.
3. Grether, W. F., Chimpanzee color vision: I. Hue discrimination at three spectral points, *J. comp. Psychol.*, 1940, **29**, 167–177.

4. GRETHER, W. F., Spectral saturation curves for chimpanzee and man, *J. exp. Psychol.*, 1941, **28**, 419–427.
5. HELMHOLTZ, H., *Treatise on physiological optics: III. The perceptions of vision* (Ed. by J. P. C. Southall), Ithaca, N. Y.: *Opt. Soc. Amer.*, 1925, pp. xix + 763.
6. HENNEMAN, R. H., A photometric study of the perception of object color, *Arch. Psychol., N. Y.*, 1935, **27**, no. 179, pp. 88.
7. HERING, E., Beitrag zur Lehre vom Simultankontrast, *Z. Psychol.*, 1890, **1**, 18–28.
8. KATZ, D., *The world of color* (Tr. by R. B. MacLeod and C. W. Fox), London: Kegan Paul, Trench, Trübner & Co., 1935, pp. x + 300.
9. KNOLL, F., Lichtsinn und Blütenbesuch des Falters von Deilephila livornica, *Z. vergl. Physiol.*, 1925, **2**, 329–380.
10. KÜHN, A., Farbenunterscheidungsvermögen der Tiere, *Handb. norm. path. Physiol.*, 1929, **12**, 720–741.
11. LASHLEY, K. S., The mechanism of vision: XV. Preliminary studies of the rat's capacity for detail vision, *J. gen. Psychol.*, 1938, **18**, 123–193.
12. LOCKE, N. M., Color constancy in the rhesus monkey and in man, *Arch. Psychol., N. Y.*, 1935, **28**, no. 193, pp. 38.
13. RÉVÉSZ, G., Tierpsychologische Untersuchungen (Versuche an Hühnern), *Z. Psychol.*, 1922, **88**, 130–137.
14. SHERRINGTON, C. S., On reciprocal action in the retina as studied by means of some rotating discs, *J. Physiol.*, 1897, **21**, 33–54.
15. TSCHERMAK, A., Licht- und Farbensinn, *Handb. norm. path. Physiol.*, 1929, **12** (part 1), 295–501.

EXPERIMENTAL EVIDENCE FOR THE ELECTRICAL CHARACTER OF VISUAL FIELDS DERIVED FROM A QUANTITATIVE ANALYSIS OF THE PONZO ILLUSION *

BY WILLIAM R. SICKLES

Teachers College, Columbia University

Little work has been done, heretofore, in attempts to define, either qualitatively or quantitatively, the nature of the field within and around a visual percept. Only two names stand out with a great degree of prominence—Lewin and Köhler. In his *Dynamics in Psychology*, published last year, the latter laid down the foundations of a theory dealing, for the first time, with the actual internal character of visual fields. It is from this work that I wish to quote. In speaking of the tendency of a figure to block its own process, he says: "Percept processes here show a behavior which is obviously analogous to that of currents in electrolytes. Can it be that percepts are actually associated with electric currents in the nervous system?" (p. 74). And though the experiment here being reported was conceived and begun prior to the publication of Köhler's question, it is, in a sense, a direct answer to it. However, it is probably not the answer anticipated—for on p. 115 of his volume, Köhler states: "The fact that all electric phenomena in the brain are of slight intensity tends to exclude electromagnetism and electrodynamics." That the situation is just the opposite is more likely in the light of what follows.

This experiment was designed to test a specific theory, this theory being based on several major assumptions. First, it was assumed that the contours of visual percepts create identical contours in the central processes; second, that these contours are the action paths of electric currents; and third, that the internal structure of such visual percepts follows in every detail the laws of the electromagnetic fields which would therby be created. Now, if one were to grant the truth of these assumptions for the purpose of experimentation, this internal structure could be predicted and calculated quantitatively, and a comparison of the curve so predicted with that obtained experimentally would enable one, consequently, to accept or reject the above assumptions. Inasmuch, however, as it is realized that these postulates are of a far-reaching character, the underlying logic shall be given. It is as follows:

* This paper was delivered before the Eastern Psychological Association, April 18, 1941.

1. Light differences, plus the nature of the receptors, determine electro-chemical differences in the retina. These cause electrical impulses to travel the *tractus opticus* to the cortex.

2. When these impulses reach the cortex, they somehow set up areas of potential difference between which areas there is an electrical interchange, *i.e.*, they produce electric vectors bringing about the same structure that existed in the retina.

3. This structural identity of the retinal and cortical images, though involving vast theoretical difficulties, is a logical assumption in light of the fact that the retina, embryologically, is nothing more than a projection of the cortex itself. Dissimilarity of their basic functional principles would, therefore, be even more difficult to explain than would the identity of these principles.

4. Since all electric vectors determine electromagnetic fields (or are determined by them), the visual fields created in the cortex will be structurally and functionally electromagnetic in character.

5. Inasmuch as all electromagnetic fields follow specific laws, and can therefore be predicted and calculated, and inasmuch as these structural and functional laws are peculiar to such fields, any proof that visual fields react in complete accordance with these same laws is also proof that these visual fields must be electromagnetic.[1]

6. Tentatively granting the electromagnetic character of these cortical images, it can be readily seen that the lines having the greatest color-brightness predominance and structural predominance will determine the field—and other parts of the structure included within this field will be acted upon and altered in accordance with the direction and intensity values, measurable in gauss, of the electromagnetic field at that particular locus.

In order to test the validity of this very elaborate and far-reaching hypothesis, the Ponzo 'angle illusion' was selected for experimentation. This illusion, used by Ponzo to explain the phenomenal difference in the apparent sizes of stars seen at the horizon compared with those higher in the sky, was selected for two reasons.[2] First, it is a visual illusion—and by the hypothesis laid down, this illusory quality is due to the intensity 'distortions' in the electromagnetic field, and it can therefore be determined and predicted; second, it is both simple and symmetrical in structure, and therefore the ratio of gaussian change in the field—with the relative movement of any point on the bisector—can be determined geometrically without

[1] Though gravity and light also function with respect to the law of the inverse square, electromagnetic fields have the strange structural property of tri-perpendicularity, *i.e.*, they are set up at right angles to the current and act at right angles to any diameter of the field so set up.

[2] *Cf.*, M. Ponzo, Rapports entre quelques illusions visuelles de contraste angulaire et l'appréciation de grandeur des astres à l'horizon, *Archives Italiennes de Biologie*, 1913, 58, 327–329.

knowing the absolute intensity of the vectors determining the field. This latter is an important feature, because it can be readily seen that it would be impossible to determine the actual electromotive force of any cerebrally located vectors. It is equally fortunate that, in this particular illusion, all the lines are straight, the structure symmetrical, and of the same degree of 'color' and brightness, and that we can therefore legitimately assume that the electric vectors created by these lines have an equal and constant intensity for any individual at a given time.

With respect to this theory of the physics involved, the Ponzo illusion creates the following situation:

1. The outer lines predominate in both structure and color-brightness and therefore determine, as vectors, the electromagnetic field whereby the two enclosed verticals lose or gain gaussian intensity value in accordance with their position on the bisector.

2. The field determined by the outer lines will be symmetrical with respect to the bisector of the angle, and therefore the loss in gauss intensity will be uniform for any point as it travels out the bisector—regardless of the angle.

3. The electromagnetic 'pull' on the enclosed verticals is of such a nature that it can be calculated as though it were concentrated on a point centrally located on the vertical where it crosses the bisector.[3]

4. These two verticals, of equal value but differently placed on the bisector of the Ponzo field, will have different final intensity values when added to or subtracted from by the Ponzo field at each individual position on this bisector. And the true, or final intensity values of these lines, when judged one by the other, will vary in direct proportion to their distance apart on the bisector, and can therefore be mathematically predicted and calculated.

5. As these enclosed verticals proceed out and beyond the bisector of the Ponzo field, they lose intensity value because of the divergence of the outer lines determining the field. Therefore, if one vertical is given, and another is drawn freehand through a point farther out the bisector, this latter line will have to be drawn longer in order to have the same psychological or phenomenal value—due to the fact that it is located in an area of the field having less intensity.

The size angle used in the experiment was 40°, the sides of the angle were 102 mm long, the standard vertical was 18 mm and was

[3] Not really a pull on the verticals, but actually a field tension in the area occupied by the verticals, this tension being normal to the side of the angle, *i.e.*, the determining vector. This question was raised by Köhler in the discussion following the presentation of this paper and clarified there.

set 26 mm from the apex of the angle. All lines were black on white paper and 1 mm in width.

Where H is the gauss value, i the constant though unknown electromotive force of the outer lines, d the distance of a given point on the bisector, R the distance between this point and either side of the angle measured perpendicular to the sides, A the angle between R and a line drawn from the point to the end of the angle-side, and B the angle between R and the bisector, then the basic formula used to calculate each position on the bisector, was as follows:

$$H = \frac{2\,i}{R}\,(\text{Sin } B - \text{Sin } A).$$

Since the above given dimensions of the angle used in this experiment were known, and constant, Sin B was known and constant, and Sin A was changed to a function of R. However, since R bore a constant relationship to d, R was changed to a function of this term—thereby reducing all measurements to simply the distance of the point on the bisector.

Nevertheless, this still left two unknown terms: H, the desired gauss value of the field occupied by the vertical, and i, the unknown electromotive force of the outer vectors. Now, inasmuch as all the lines of the illusion were of the same width and color-brightness, *i.e.*, had the same structural properties, it was felt that their functional properties (so far as electromotive force was concerned) would also be the same for any given individual at a given time. Therefore, the desired values in gauss, H, were obtained in terms of the constant i.

In order to get the field tension measured parallel to the verticals, *i.e.*, perpendicular to the bisector, rather than perpendicular to the sides of the angle, the values obtained for H were multiplied by the value for Sin B, and the resulting formula, for the particular dimensions used in this experiment, then became:

$$\frac{H}{i} = \frac{1.77}{.342\,d} - \frac{.603\,d - 65.582}{.342\,d\,\sqrt{(.321\,d - 34.884)^2 + (.117\,d)^2}}.$$

With this formula, then, eighteen different positions on the bisector, and beyond, were calculated for the desired gauss value in terms of the constant, though unknown, electromotive force. The dynamics of the Ponzo field were therefore determined *a priori* according to the principles of electromagnetic activity, and the curve expressing this was predicted to be the same curve which would be obtained experimentally for the actual phenomenal distortion occurring within the illusion. This curve (*Cf.* Fig. 1), predicted mathe-

matically from the physics of the field, was seen to be an asymptotic hyperbola—not an unusual type of curve for electrodynamical processes. The region in which it became seriously asymptotic lay in that part of the Ponzo field approximately 141 mm from the apex of the angle used—or approximately 115 mm from the standard vertical which was set aribtrarily at 26 mm from the apex of the angle.

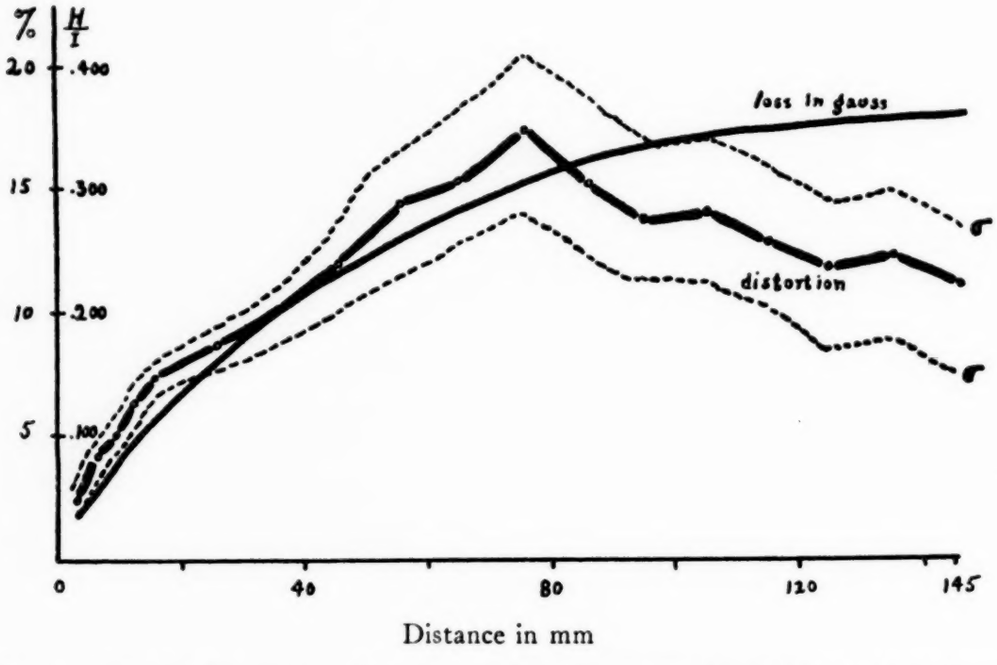


FIG. 1. Loss in electromagnetic intensity of the Ponzo Field, relative to the bisector, as contrasted with the phenomenal distortion in the same field.

Since the intensity was constant, these values were obtained in terms of the gauss value divided by the intensity, or, $\frac{H}{i}$. The gauss value of the standard was then subtracted from the gauss value of each of the other eighteen points on the bisector, in order to obtain the degree of gaussian difference relative to positional differences on the bisector—it being predicted that these electromagnetic field differences were actually the amount of the distortion perceived by the subjects.

Having predicted, from purely physical considerations, the degree of phenomenal distortion relative to positional differences on the bisector of the Ponzo illusion, this distortion, relative to these same positions, was determined experimentally. Sheets of white paper were printed with the angle and one vertical, set on the bisector 26 mm from the apex of the angle—the bisector not being printed. The angle selected was 40°. All lines were black and a millimeter in width, the vertical, having the same width, was 18 mm long. The sides of the angle were 102 mm long.

Thus, the subjects were given the illusion with only one vertical present. A dot was placed on the bisector of each angle, and they were told to draw a line, freehand, through this dot—and to make the line so drawn equal in length and parallel to the given vertical. The instructions were as follows: "Here is a package of papers. On each of these papers is printed an angle the general orientation of which is parallel to the eyes. Inside the angle is a vertical line. Further

out will be seen a dot. I want you to draw, through this dot, another line equal in length and parallel to the one printed. Please do not use any means of measurement, nor shift the papers from their present perspective to obtain a different view, nor shift your head for the same purpose. To do so is to ruin the experiment. The line is to be drawn entirely freehand. As you go through the papers you will find that the position of the dot will vary."

Fifteen subjects were used, all college students, none of whom had any training in art. They were carefully watched to see that the instructions were fulfilled, and all of them were found to coöperate with complete honesty. Each subject drew ten such lines at each of eighteen positions on the bisector. No time limit was set, and the work was frequently interrupted to prevent fatigue.

It was assumed that the errors would follow a normal distribution, and that the mean of these errors would represent the degree of visual distortion. The difficulties involved in this Method of Average Error were, of course, recognized. The subjects were divided into three groups, consequently, and each group received a different series. These series were: an ascending, a descending, and an alternating. All these data, so obtained, were combined in determining the final curve (*Cf.* Fig. 1). Therefore, in order to get a larger sampling wherefrom to draw the means, 2700 separate judgments were used in drawing the experimentally obtained curve—150 judgments at each of 18 positions on the bisector, these positions proceeding out the field from 3 mm from the standard vertical to 145 mm, *i.e.*, from very close within the field to a position completely beyond the angle and out of the field.

For his invaluable aid in working out the experimental procedure, obtaining the subjects, and, in short, collaborating with me in every possible way, I am indebted to Schwarzbek of Wittenberg College. The experiment is, therefore, as much his as my own. Subsequently, Schwarzbek worked out a check experiment wherein the standard was placed outside the angle at various positions on the bisector, and the judged line drawn inside at a constantly fixed dot. For reasons of methodology, however, these data could not be compared directly with those of the initial experiment.[4] These data, then, are omitted.

And now, as to the results of the experiment, it will be noticed from Fig. 1 that the smooth curve expressing the predicted distortion, in terms of difference in gaussian intensity divided by the unknown, though constant, electromotive force, closely approximates the irregular experimentally obtained curve.

The very fact that these two curves are of the same general class is surprising enough, but it is truly astonishing to note that the smooth predicted curve follows the irregular experimental curve very closely as far as the modal point of the latter. Here, where the

[4] Schwarzbek writes: "In my opinion, the basic assumption underlying the method is faulty. This is because the subjects, in spite of any effort to the contrary, once they established a 'set,' always drew the lines so the extremes were a constant distance from the sides of the angle."

electromagnetic field would be weakest, the predicted curve maintains an approximate level (being asymptotic to the X axis), and the experimental curve becomes much more irregular and drops slightly.

It is highly significant that the point where the predicted curve levels off is almost equivalent to the modal point of the experimental curve. The fact that the latter, having reached this point, drops slightly and becomes more irregular, is to be expected. At this point the distortion is at its maximum, and the field intensity at its minimum. Though this point would not be difficult to reach, the difficulties involved in making judgments at this level of maximum distortion render it highly improbable that it could be maintained in the same manner as that wherein the predicted hyperbola levels off. This is verified in the fact that the standard deviations of the experimental curve become increasingly much larger once this point is reached. Further evidence is seen in that there are no regressions, or 'drops' in the experimental curve prior to this point. Afterwards, however, there are several such regressions.[5]

A comparison of these curves immediately reveals a very significant feature, viz., that as far as the point of maximum distortion, they are very closely related. So close is this relationship that one experimentally obtained value coincides exactly with that predicted! And except for a brief initial distance, the one stays within the standard deviations of the other. The point, however, at which the values coincide exactly, is extremely interesting. Statistically speaking, the probability of this happening on the basis of sheer chance would be astronomical in its calculation— especially when one considers the fact that there was no statistical 'juggling' of the data wherefrom these curves were obtained, no 'curve fitting,' and no introduction of spurious 'constants' to alter the shape of the curves—a trick often resorted to by those who have an irresistible urge to support their views. These curves were drawn from the data, *per se*.

Equally interesting is the fact that many of the individual verticals drawn by the subjects tended, when not centrally placed on the bisector, to tip or lean towards a position perpendicular to the sides of the angle. That is, the lines so drawn were not parallel with the standard line but sought perpendicularity with the side of the angle—this direction of perpendicularity being the direction of magnetic action of that side. When the greater proportion of the vertical was above the bisector, they tipped perpendicular to the upper side of the angle, and when below the bisector, to the lower

[5] After further consideration, it can be seen that other factors may be involved in accounting for this drop in the experimental curve—since at no point does the distortion again reach the maximum. Such factors would be: the time involved in further eye-movement, visual span, or actual *E.M.U.* differences in the periphera of the retina.

side of the angle. No 'reverse cases' of tipping were found, and better than 17 percent of the lines drawn revealed this phenomenon.

From this experiment, then, the following conclusion is drawn, viz., that despite its specific and restricted nature, inasmuch as it was an experiment with the results predicted beforehand, and inasmuch as these results fulfill, for the most part, this prediction, it is highly probable that visual fields are brought about in the central processes by actual electric currents, and that their internal structure, and their coherence, and their distortions, literally follow the familiar principles of electromagnetism.

Many other similar instances of visual distortion are open, by the same theory, to prediction and investigation. It is felt that the results of subsequent experimentation would confirm the theory reported here.

(Manuscript received June 3, 1941)